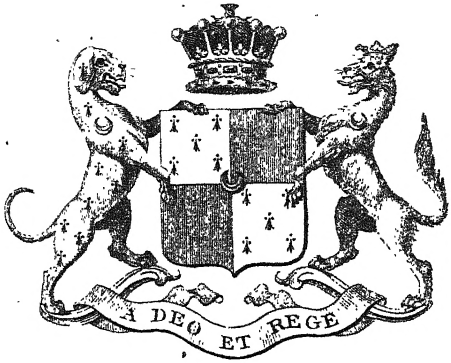
A DEO ET REGE

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCVIII.

PART I.

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MDCCXCVIII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds

of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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APPENDIX.

PHILOSOPHICAL

TRANSACTIONS.

I. *The* Bakerian *Lecture. Experiments upon the Resistance of Bodies moving in Fluids. By the Rev.* Samuel Vince, *A. M. F. R. S. Plumian Professor of Astronomy and Experimental Philosophy in the University of Cambridge.*

Read November 9, 1797.

In a former Paper upon the Motion of Fluids, I stated the difficulties to which the theory is subject, and showed its insufficiency to determine the time of emptying vessels, even in the most simple cases; I also proved, by actual experiments, that, in many instances, there was no agreement between their results and those deduced from theory. The great difference between the experimental and theoretical conclusions, in most of the cases which respect the times in which vessels empty themselves through pipes, necessarily leads us to suspect the truth of the theory of the action of fluids under all other circumstances. In the doctrine of the resistances of fluids, we see strong reasons to induce us to believe, that the theory cannot generally lead us to any true conclusions. When a body

moves in a fluid, its particles strike the body; and, in our theoretical considerations, after this action, the particles are supposed to produce no further effect, but are conceived to be, as it were, annihilated. But, in fact, this cannot be the case; and what we are to allow for their effect afterwards, is beyond the reach of mere theoretical investigation. Whatever theory therefore we can admit, must be that which is founded upon such experiments as include in them every principle which is subject to any degree of uncertainty. We must therefore have recourse to experiments, in order to establish any conclusions upon which we may afterwards reason. In the paper above mentioned, I described a machine to find the resistances of bodies moving in fluids; since which time, I have made a variety of experiments with it, upon bodies moving both in air and water, and I have every reason to be satisfied of its great accuracy. In this paper, I propose to examine the resistance which arises from the action of non-elastic fluids upon bodies.

This subject divides itself into two parts; we may consider the action of water at rest upon a body moving in it, or we may consider the action of the water in motion upon the body at rest. We will first give the result of our experiments in the former case, and compare them with the conclusions deduced from theory. Now the radius of the axis of the machine made use of in these experiments was 0,2117 in. the area of the four planes was 3,73 in. the distance of their centres of resistance from the axis was 7,57 in. and they moved with a velocity of 0,66 feet in a second. The first column of the following table exhibits the angles at which the planes struck the fluid; the second column shows the resistance by experiment, in the direction of their motion, in Troy ounces; the third column

gives the resistance by theory, assuming the perpendicular resistance to be the same as by experiment: the fourth column shows the power of the sine of the angle to which the resistance is proportional.

Angle.	Experiment.	Theory.	Power.
10°	0,0112	0,0012	1,73
20	0,0364	0,0093	1,73
30	0,0769	0,0290	1,54
40	0,1174	0,0616	1,54
50	0,1552	0,1043	1,51
60	0,1902	0,1476	1,38
70	0,2125	0,1926	1,42
80	0,2237	0,2217	2,41
90	0,2321	0,2321	

The fourth column was thus computed: Let s be the sine of the angle to radius unity, r the resistance at that angle, and suppose r to vary as s^m; then $1^m : s^m :: 0{,}2321 : r$, hence, $s^m = \frac{r}{0{,}2321}$, and consequently $m = \frac{\log. r - \log. 0{,}2321}{\log. s}$; and, by substituting for r and s their several corresponding values, we get the respective values of m, which are the numbers in the fourth column. Now the theory supposes the resistance to vary as the cube of the sine; whereas, the resistance decreases from an angle of 90°, in a less ratio than that, but not as any constant power of the sine, nor as any function of the sine and cosine, that I have yet discovered. Hence, the actual resistance is always greater than that which is deduced from theory, assuming the perpendicular resistance to be the same; the reason of which, in part at least, is, that in our theory we neglect the

whole of that part of the force which, after resolution, acts parallel to the plane; whereas (from the experiments which will be afterwards mentioned), it appears that part of that force acts upon the plane; also, the resistance of the fluid which escapes from the plane, into the surrounding fluid, may probably tend to increase the *actual* resistance above that which the theory gives, in which that consideration does not enter; but, as this latter circumstance affects the resistance at all angles, and we do not know the quantity of effect which it produces, we cannot say how it may affect the *ratio* of the resistances at different angles.

In theory, the resistance perpendicular to the planes is supposed to be equal to the weight of a column of fluid, whose base = 3,73 in. and altitude = the space through which a body must fall to acquire the velocity of 0,66 feet; now that space is 0,08124 in. consequently the weight of the column = 0,1598 Troy oz.; but the actual resistance was found to be = 0,2321 oz. Hence, the actual resistance of the planes : the resistance in our theory :: 0,2321 : 0,1598, which is nearly as 3 : 2.

I am aware that experiments have been made upon the resistances of bodies moving in water, which have agreed with our theory. An extensive set was instituted by D'ALEMBERT, CONDORCET, and BOSSUT, the result of which very nearly coincided with theory, so far as regards the absolute quantity of the perpendicular resistance. Their experiments were made upon floating bodies, drawn upon the fluid by a force acting upon them in a direction parallel to the surface of the fluid. There can be no doubt but that these experiments were very accurately made. The experiments here related were also repeated so often, and with so much care, and the results always

agreed so nearly, that there can be no doubt but that they give the actual resistance to a very considerable degree of accuracy. In our experiments, the planes were immersed at some depth in the fluid; in the other case, the bodies floated on the surface; and I can see no way of accounting for the difference of the resistances, but by supposing that, at the surface of the fluid, the fluid from the end of the body may escape more easily than when the body is immersed below the surface; but this, I confess, appears by no means a satisfactory solution of the difficulty. The resistances of bodies descending in fluids manifestly come under the case of our experiments.

Two semi-globes were next taken, and made to revolve with their flat sides forwards. The diameter of each was 1,1 in. the distance of the centre of resistance from the axis was 6,22 in. and they moved with a velocity of 0,542 feet in a second; and the resistance was found to be 0,08339 oz. by experiment. By theory, the resistance is 0,05496 oz.; hence, the resistance by experiment : the resistance by theory :: 0,08339 : 0,05496, agreeing very well with the abovementioned proportion. But, when the spherical sides moved forwards with the same velocity, the resistance was 0,034 oz. Hence, the resistance on the spherical side of a semi-globe : resistance on its base :: 0,034 : 0,08339; but this is not the proportion of the resistance of a perfect globe to the resistance of a cylinder of the same diameter, moving with the same velocity, because the resistance depends upon the figure of the back part of the body.

I therefore took two cylinders, of the same diameter as the two semi-globes, and of the same weight; and, giving them the same velocity, I found the resistance to be 0,07998 oz.;

therefore the resistance on the flat side of a semi-globe : the resistance of a cylinder of the same diameter, and moving with the same velocity :: 0,08339 : 0,07998. This difference can arise only from the action of the fluid on the back side of the semi-globe, moving with its flat side forwards, being less than that on the back of the cylinder, in consequence of which the semi-globe suffered the greater resistance. The resistance of the cylinders, thus determined directly by experiment, agrees very well with the foregoing experiments. The resistance, *cæteris paribus*, varies as the square of the velocity very nearly, and may be taken so for all practical purposes, as I find by repeated experiments, made both upon air and water, in the manner described in my former paper. Hence, for different planes, the resistance varies as the area × the square of the velocity. Now the resistance of the planes whose area was 3,73 in. moving with a velocity of 0,66 feet in a second, was found to be = 0,2321 oz. Also, the area of the two cylinders was 1,9 in. and their velocity was 0,542 feet in a second; to find, therefore, the resistance of the cylinders from that of the planes, we have $0,66^2 \times 3,73 : 0,542^2 \times 1,9 :: 0,2321$ oz : 0,07973 oz. for the resistance on the cylinders, differing but a very little from 0,07998 oz. the resistance found from direct experiment.

Now, to get the resistance on a perfect globe, we must consider, that when the back part is spherical, the resistance is greater than when it is flat, in the ratio of 0,08339 : 07998; hence, the resistance on a globe : the resistance on a semi-globe in the same ratio; but the resistance on the semi-globe was 0,034 oz. hence, 0,07998 : 0,08339 :: 0,034 oz. : 0,0354 oz. the resistance of a globe; consequently, the resistance of a globe : the resistance of a cylinder of the same diameter, mov-

ing with the same velocity in water :: 0,0354 : 0,07998 :: 1 : 2,23.

We proceed next to compare the actual resistance of a globe with the resistance assumed in our theory. In the first place, the absolute quantity of resistance has been found to be greater than that which we use in theory, in the ratio of 0,2321 : 0,1598; but, by theory, the resistance of the globe : the resistance of the cylinder :: 1 : 2, or as 1,115 : 2,23; hence, by theory, we make the resistance of the globe too great, in the ratio of 1,115 : 1; and it is too small, from the former consideration, in the ratio of 0,1598 : 0,2321; therefore the actual resistance of the globe : the resistance in theory :: 0,2321 : 0,1598 × 1,115 :: 0,2321 : 0,1782, which is nearly in the ratio of 4 : 3. Thus far we have considered the resistance of bodies moving in a fluid; we come next to consider the action of a fluid in motion upon a body at rest.

A vessel 5 feet high was filled with a fluid, which could be discharged by a stop-cock, in a direction parallel to the horizon. The cock being opened, the curve which the stream described was marked out upon a plane set perpendicular to the horizon; and, by examining this curve, it was found to be a very accurate parabola, the abscissa of which was 13,85 in. and the ordinate was 50 in. hence, the latus rectum was 180,5 in. one-fourth of which is 45,1 in. which is the space through which a body must fall to acquire the velocity of projection; hence, that velocity was 189,6 in. in a second. And here, by the by, we may take notice of a remarkable circumstance. The depth of the cock below the surface of the fluid was 45,1 in. hence, the velocity of projection was that which a body acquires in falling through a space equal to the whole depth of the fluid; whereas,

through a simple orifice, the velocity would have been that which is acquired in falling through half the depth; the pipe of the stop-cock therefore increased the velocity of the fluid in the ratio of $1 : \sqrt{2}$, and gave it the greatest velocity possible; the length of the pipe was 3 in. and the area of the section 0,045 in.; also, the base of the vessel was a square, the side of which was 12 inches.

The area of the section of the pipe may be found very accurately, in the following manner. The vessel being kept constantly full, receive the quantity of fluid run out in any time t'', and then weigh it, by which we shall be able to get the quantity in cubic inches. Now if $v =$ the velocity of the fluid when it issues from the pipe, $a =$ the area of the section of the pipe, $l =$ the length of the cylinder of water run out, whose base $= a$, and $m =$ the quantity of fluid discharged in t''; then $v : l :: 1'' : t''$, hence, $l = vt$; but $al = m$; therefore $avt = m$; hence, $a = \frac{m}{vt}$. In the present instance, $t = 20$, $m = 170,63$ cubic inches, $v = 189,6$; hence, $a = 0,045$.

Let ABCD (fig. 1. Tab. I.) be a solid piece of wood, upon which are fixed two upright pieces, *rs*, *tu*; between these, a flat lever *eac* is suspended, in a perpendicular position, on the axis *xy*, and nicely balanced; and let *a* be a point directly against the middle of the axis, in a line perpendicular to the plane of the lever. This apparatus is placed against the stop-cock, at the distance of about 1 inch, and, when the water is let go, let us suppose the centre of the stream to strike the lever perpendicularly at *e*; take $ac = ae$, and, on the opposite side to that at which the stream acts, fasten a fine silk string at *c*, and bring it over a pulley *p*, and adjust it in a direction perpendicular to the plane of the lever, and, at the end which hangs

down, fix a scale Q, the weight of which is to be previously determined. All the apparatus being thus adjusted, open the stop-cock, and let the fluid strike the lever, and put such weight into the scale as will just keep the lever in its perpendicular situation, and that weight, with the weight of the scale, must be just equivalent to the action of the fluid. Thus we get the perpendicular effect of the water. Now incline the plane of the lever, at any angle, to the direction of the stream, and adjust the string perpendicular to the plane, as before; then put such a weight into the scale as will keep the lever perpendicular to the horizon, whilst the fluid acts upon it, and you get that part of the effect of the fluid which acts perpendicular to the plane. In this manner, when the fluid acts oblique to the plane, we get the perpendicular part of the force. The second column of the following table shows this effect, by experiment, for every 10th degree of inclination shown in the first column; and the third column shows the effect, by theory, from the perpendicular force, supposing it to vary as the sine of inclination.

Angle	Experiment.			Theory.		
	oz.	dwts.	grs.	oz.	dwts.	grs.
90°	1	17	12	1	17	12
80	1	17	0	1	16	22
70	1	15	12	1	15	6
60	1	12	12	1	12	11
50	1	18	10	1	18	17
40	1	4	10	1	4	2
30	0	18	18	0	18	18
20	0	12	12	0	12	19
10	0	6	4	0	6	12

It appears from hence, that the resistance varies as the sine of the angle at which the fluid strikes the plane; the difference between the theory and experiment being only such as may be supposed to arise from the want of accuracy to which the experiments must necessarily be subject.

Let us now first consider, what the whole perpendicular resistance by experiment is, when compared with that by theory. Now, by theory, the resistance is equal to the weight of a column of the fluid, whose base = 0,045 in. and altitude = 45,1 in. and the weight of that column is = 1 oz. 1 dwt. 10 grs. Hence, the resistance by theory : the resistance by experiment : : 1 oz. 1 dwt. 10 grs. : 1 oz. 17 dwts. 12 grs. : : $514\frac{1}{4}$: 900.

In the next place, let us examine what is this resistance, compared with the resistance of a plane moving in a fluid. We here prove, that the resistance of the fluid in motion acting on the plane at rest : the resistance by theory : : 900 : 514; and we have before proved, that the resistance by theory : the resistance of a plane body moving in a fluid : : 1598 : 2321; hence, the resistance of a fluid in motion upon a plane at rest : the resistance of the same plane, moving with the same velocity, in a fluid at rest : : 900 × 1598 : 514 × 2321 : : 1438200 : 1192954 : : 6 : 5 nearly. Now we know that the actual effect on the plane must be the same in both cases; and the difference, I conceive, can arise only from the action of the fluid behind the body, in the latter case, there being no effect of this kind in the former case. For, in respect to the pressure before the body, that will probably be the same in both cases; for there is a pressure of the column of the spouting fluid, acting against

the particles which strike the body at rest, similar to the action of the fluid before the body, upon the particles which strike the body moving in the fluid. Hence, the resistance of the planes moving in the fluid, with the velocity here given, is diminished about one fifth part of the whole, by the pressure behind the body; but, with different velocities, this diminution must increase as the velocity increases.

The effect of that part of the force which acts *perpendicular* to the plane being thus established, we proceed next to examine, what part of the whole force which acts *parallel* to the plane, is effective. To determine which, the axis *w v* (fig. 2.) was fixed perpendicular to the plane of the lever *a b c d*, and the ends of the axis were conical, and laid in conical holes; and the thread from which the scale was hung was fixed to the edge at *e*, and acted perpendicular to it and the weight drew the lever in the direction *e s*, contrary to that in which the fluid tends to move the lever, and it acted at the same perpendicular distance from the axis below, as the fluid acted above it. Let *x m z* be a line parallel to the horizon, when the lever is perpendicular to it, and which passes through the centre of the stream; and let *x m z* be also the direction of that part of the force which acts parallel to the plane. This apparatus being adjusted, the experiments were made for every tenth degree of inclination; and here a circumstance took place, for which I can give no satisfactory reason. Having gone through the experiments once, and noted the results, I repeated them; and, to my great surprise, I found all the second results to be very different from the first. The experiments were therefore repeated again, and the results were still different. Being certain that the experiments were very accurately made each time, I

was totally at a loss to conjecture to what circumstance this difference of results was owing. By repeating however the experiments, and observing at what point of the line $x\,m\,z$ the centre of the stream acted, I discovered that the effect varied by varying that point; that it was greatest when the stream struck the lever as near as it could to x; less when it struck it at the middle m; and least when it struck it as near as it could to z, notwithstanding the stream acted at the same perpendicular distance from the axis in each case, and the parallel part of the force always acted in the line $x\,m\,z$. At the angles 80°, 70°, 60°, the fluid striking as near as it could to the edge z, gave the lever a motion, not in the direction $x\,m\,z$, but in the opposite direction $z\,m\,x$, as appeared by taking away the scale. I have therefore marked such results with the sign ——, the motion produced being then in a direction opposite to that which ought to have been produced, by that part of the force of the stream which acts parallel to the plane of the lever. The forces which are here put down, are those which take effect in a direction parallel to the plane of the lever, for every tenth degree of inclination; the perpendicular force being 1 oz. 17 dwts. 12 grs.

		dwts.	grs.
At 80° incl.	Edge z	——	
	Middle m	3	3
	Edge x	10	17
At 70° incl.	Edge z	——	
	Middle m	6	2
	Edge x	11	10
At 60° incl.	Edge z	——	
	Middle m	7	9
	Edge x	11	22

		dwts.	grs.
At 50° incl.	Edge z - -	0	17
	Middle m -	8	20
	Edge x - -	13	21
At 40° incl.	Edge z - -	1	16
	Middle m -	8	6
	Edge x - -	13	15
At 30° incl.	Edge z - -	3	20
	Middle m -	7	2
	Edge x - -	12	15
At 20° incl.	Edge z - -	4	16
	Middle m -	6	0
	Edge x - -	11	12
At 10° incl.	Middle m -	5	12

It is a remarkable circumstance, that the effect of the fluid at z increased regularly as the angle decreased; for, though I did not measure the negative effects, I could plainly perceive that that was the case; whereas, the effects at m and x increased to about the middle of the quadrant, and then decreased. At 10°, the obliquity was such, that the section of the stream extended very nearly from one side of the lever to the other.

As it appears by experiment, that the velocity of the fluid flowing out of the vessel was equal to the velocity which a body acquires in falling down the altitude of the fluid above the orifice, the square of the velocity must be in proportion to that altitude. To find therefore, in this case, whether the resistance varied as the square of the velocity, I let the water flow per-

pendicularly against the plane (fig. 1.) at different depths, and I always found the resistances to be in proportion to the depths, and therefore in proportion to the square of the velocity, agreeing with what takes place when the body moves in the fluid.

Fig. 1.

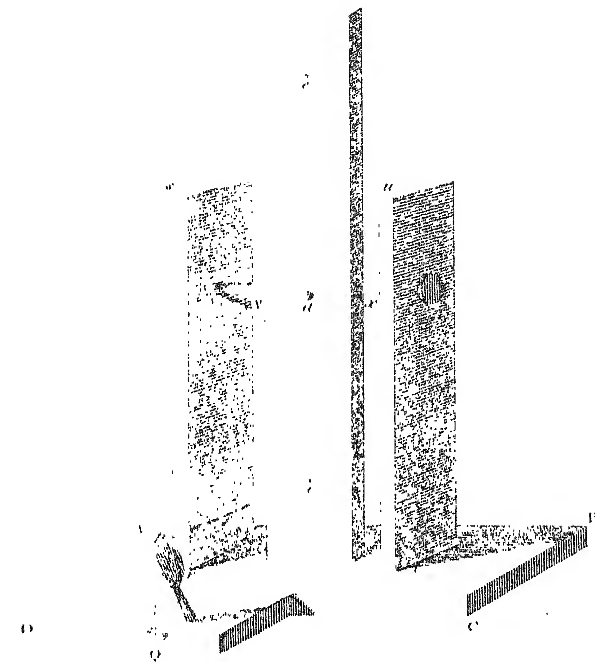
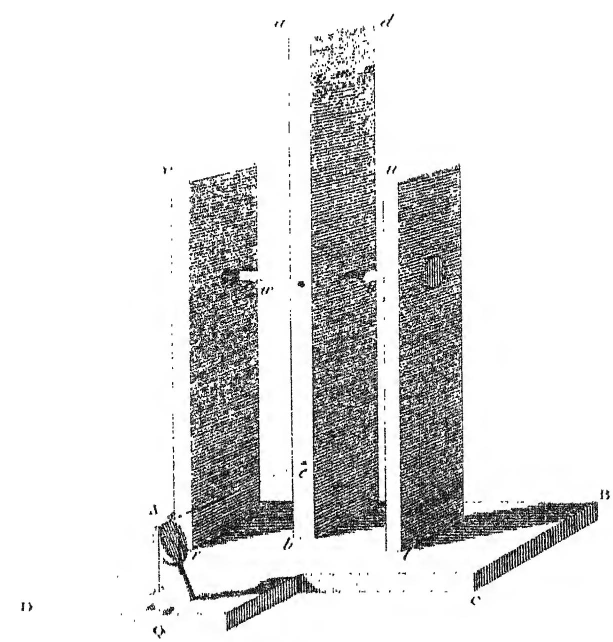


Fig. 2.



II. *Experiments and Observations, tending to show the Composition and Properties of Urinary Concretions.* By George Pearson, *M. D. F. R. S.*

Read December 14, 1797.

I. *Historical Observations.*

URINARY concretions have obtained their denominations, like most other things, from their obvious properties. Accordingly, in our language, they are popularly known by the names Stone and Gravel, or Sand, from their resemblance to the states of earth so named: and we find names of the same import in other languages, such as λιθος, (ARETÆUS;) λιθιασις, (CÆLIUS AURELIANUS;) ψαμμος, (ARETÆUS;) λιθιδια, (various authors;) *Calculus,* (CELSUS and PLINY;) *Sabulum,* (various authors.) In other languages, and especially in those now spoken, it is unnecessary to notice names which have the same meaning.

The notion very generally entertained, of the nature of urinary concretions, consisted with the terms, till the last twenty years; although the experiments of SLARE, FREDERIC HOFFMAN, and HALES, long before showed that these substances commonly consist of animal matter. GALEN indeed imagined that φλεγμα, or viscid animal matter, is the basis of animal concretions; but, in his days, earth was believed to be the basis of animal matter. Alkaline medicines were, however, employed by the Greek physicians, in diseases from calculi.

The experiments of the alchemists also made it appear, that earth was only a part of the matter of concretions. It was probably the observation of the deposition and crystallization of saline bodies, which suggested the notion of urinary calculi being of the nature of tartar. Such was the opinion of BASIL VALENTINE, and after him of HOCHENER, better known by the name of PARACELSUS; but, whether the latter adopted the denomination *Duelech* from its import, or from caprice, has not been explained. VAN HELMONT, a century after his prototype PARACELSUS, being struck with the experiment in which he discovered the concretion of salts in distilled urine by alcohol, was led to depart from his adored master's opinion, with respect to the nature of calculi; although he acknowledges the merit of PARACELSUS, in having discovered the solvent *Ludus*, (a calcareous stone also called *Septarium*,) which VAN HELMONT says is preferable to alkaline lixivium. He also says, that when the archeus spirit of urine meets with a volatile earthy spirit, and does not act in a due manner, a concretion will be formed; but, in a healthy state, although all urine contains the matter of urinary calculi, no concretion can take place, because the archeus, or vital power of the bladder, counteracts its formation.

As to the kind of earth composing calculi, the only distinction of earths, till about the last half century, was into absorbent and non-absorbent; but, since the absorbent earths were distinguished into calcareous, magnesia, and alumine or clay, the calcareous was considered to be the earth of urinary concretions; apparently however for no other reason but its obvious properties, and its extensive diffusion through the whole animal kingdom.

At length, *viz.* in 1776, the experiments of the wonderful SCHEELE were published in Sweden, but were scarcely known in this country till 1785. These experiments exploded the opinion of the earthy nature of calculi, and substituted that of their consisting of a peculiar acid, resembling the succinic, and of a gelatinous matter, without any earth. Afterwards about $\frac{1}{200}$ of their weight of lime was found by BERGMAN; which, for a cause now well known, had eluded the acuteness of SCHEELE. Although the experiments of SCHEELE were confessedly unquestionable, and were ably supported by the learned BERGMAN, some very eminent chemists, having obtained different results by their own experiments, adopted a different opinion of the composition of these concretions. The immortal, and ever to be deplored, LAVOISIER supposed these substances to consist of acidulous phosphate of lime and animal matter, many of them being partially fusible; but still it was the unrivalled SCHEELE who discovered, that the urine of healthy persons contains superphosphate, or acidulous phosphate, of lime; and who also indicated the experiment which verified his opinion, that phosphate of lime is the basis of bone.

Experiments have been likewise made, for the most part in a rather desultory way, and most of them by persons but little practiced in chemical inquiries, which at least afford evidence, that urinary concretions are very different, with respect to the proportion of the ingredients in their composition, and perhaps also in kind. M. FOURCROY, who however must not be classed with inexperienced chemists, I believe first obtained prussic acid by fire, and by nitric acid, from these concretions; and showed that they sometimes contain phosphate of ammoniac and of soda; which may be dissolved out of them by

water. M. FOURCROY also says, he found magnesia in the intestinal calculus of a horse; which calculus was a triple combination, of one part of phosphate of ammoniac, two parts of magnesia, and one of water, besides traces of animal and vegetable matter.

Dr. LINK, in a very elaborate dissertation, published at Gottingen, in 1788, on urine and calculi, concludes that urinary concretions consist of phosphoric acid, lime, ammoniac, oil, the bases of different kinds of gazes, together with the acid sublimate of SCHEELE, although he did not succeed in obtaining it.

It is a proof of Dr. BLACK's sagacity, that he should have been able to perceive, from a few experiments, that urinary concretions consisted of animal matter and the earth of bone, before the composition of this earth was demonstrated by GAHN.

In this historical sketch it should be noticed, that alkaline substances, though used by the Greek physicians, and afterwards by the alchemical physicians, appear to have been laid aside by the regular practitioners, for a century or two preceding their revival, by the famous Mrs. STEPHENS, in 1720. Her prescription brought into vogue the theory of these medicines operating by their causticity. The successful use, by Mr. COLBORNE, of potash saturated with carbonic acid, according to the discovery of BEWLEY and BERGMAN, and the still further improvement in practice, from the use of soda, as well as potash, super-saturated with carbonic acid, by the discovery of a peculiar method by Mr. SCHWEPPE, have completely refuted the theory of the agency of alkalies on the principle of causticity.

It appears, from the preceding brief history, as well as from the confession of the latest and best writers, that the experi-

ments hitherto made, rather "afford indications of what re-"mains to be done, than furnish demonstrations of the nature "of animal concretions." It is also too obvious to need explanation, that more efficacious and innocent practice, in diseases from these concretions, can only be discovered by a further investigation of their properties. It is with this view, as well as for the sake of chemical philosophy, that I think it my duty to submit to the Society some of the observations I have made, in the course of inquiry on this subject.

The observations which I shall now offer, are principally on a substance, which my experiments inform me is very generally a constituent of both urinary and arthritic concretions. It is a substance obtained by dissolving it out of these concretions, by lye of caustic fixed alkali, and precipitating it from the solution by acids. In this way, SCHEELE separated this matter; but he did not consider its importance, nor of course at all investigate its properties. He does not even seem to have been aware that it was a distinct constituent part of the urinary concretion; for, when he relates the experiment of precipitating matter from the nitric solution of calculus by metallic salts, no distinction is made between the precipitations in this experiment, and that in the former; yet we can now show, that in the one case the precipitate is a peculiar animal oxide, and in the other they are metallic phosphates. As SCHEELE obtained an acid sublimate, it has been imagined by some writers, that the precipitate by any acid (even by the carbonic) from the alkaline menstruum, was an acid; the same as that obtained by sublimation, and which, in the new system of chemistry, has been denominated *lithic acid*. The following experiments show that these substances are different species of matter.

II. EXPERIMENTS.

250 grains of a white, smooth, laminated, urinary calculus, and the same quantity of a nut-brown one, with an uneven surface, both of which were of a roundish figure, were pulverized together.* 300 grains of these pulverized calculi were triturated with three ounces and a half, by measure, or five ounces, by weight, of lye of caustic soda. The mixture became thick, and copiously emitted ammoniacal gaz. After digestion for a night, and then boiling, with the addition of five ounces of pure water, I obtained, by filtration, five ounces of clear colourless liquid. Boiling water was repeatedly poured upon the strainer, till what passed through it was almost tasteless, and remained clear, on the addition of diluted sulphuric acid.

(*a*) The matter remaining on the strainer, being dried, was an impalpable, white, tasteless, heavy powder, which weighed 96 grains.

(*b*) The five ounces of filtrated liquid, having been set apart, on standing, deposited a white, opaque, granulated, soap-like matter, from a colourless clear liquid. The liquid being decanted, the deposit was dried, and was then an opaque, brittle, soap-like matter, which dissolved readily in water, giving a clear but not viscid solution, and tasting weakly of soda. This soap-like matter weighed 280 grains.

(*c*) The decanted liquor, (*b*,) being mixed with the above filtrated liquors, on evaporation to three ounces, afforded no deposit on standing, although it was a very heavy and soapy

* The object of these experiments being principally to investigate the properties of one of the constituent parts of urinary concretions, which part was previously determined (by the test of nitric acid,) to exist in both these, it can be no objection to the experiments, that I made use of a mixture of two calculi.

liquid to the feel; but, on adding diluted sulphuric acid gradually, till it ceased to become turbid, a sediment was deposited, which was a very light, white, impalpable powder, in weight, when dried, 26 grains. The liquid from which this powder was precipitated, being evaporated, afforded nothing but sulphate of soda, and a few grains of crystals, which seemed to be phosphate of soda. There was also a blackish matter, which burnt like horn, or other animal matter, and did not leave a pink or rose-coloured matter, on evaporating the solution of it in nitric acid to dryness, but left a carbonaceous residue; whereas, the white precipitate, so treated, afforded a beautiful pink matter.

(*d*) 250 grains of the soap-like matter (*b*) being dissolved in eight ounces of pure water;

1. A little of this solution, further diluted by one ounce of water, grew milky on adding a few drops of nitric acid, but became less so on standing. On adding more nitric acid, and heating it, the mixture became quite clear: by adding a few drops of lye of caustic soda, a very slight curdy appearance took place.

2. On adding, to the same diluted solution, a little of the diluted sulphuric or muriatic acid, milkiness ensued, and remained, although the acids were added till the mixture was extremely sour. On adding lye of caustic soda, much more than to saturate the superabundant acid, the mixture became clear again; and, on adding the acids a second time, the milkiness was reproduced. It was found that the milkiness could be produced and destroyed, or clearness be produced, by the alternate addition of the acid and alkali, for an unlimited number of times. If the nitric acid however was used, at length

no milkiness could be induced. If carbonate of soda was added, in place of the caustic soda, the mixture could not be made clear.

3. Lime water was rendered turbid by this solution, but I neglected to examine the precipitated matter.

4. A little of the solution, with the addition of a few drops of concentrated nitric acid, being evaporated to dryness, sometimes a pink, and at other times a blood-red, or rose-coloured matter was left; which, by further application of fire, became black.

5. Carbonic acid, digested and shook with this solution, did not render it turbid.

6. To the whole of the remaining solution was added diluted sulphuric acid, to saturate the alkali. On standing, a copious precipitate took place, from a clear liquid; which precipitate, being washed and dried, was a mass of very light, mica-like, whitish crystals, amounting to 123 grains. It was estimated that the solution used in the Experiments 1.—5. would have produced 12 grains, and that the 30 grains of soap-like matter, (*b*,) not decompounded, would have yielded about 14 grains more.

(*e*) The precipitate, (*d*, 6.)

1. Had no taste, nor smell, and did not dissolve in the mouth.

2. About one part of it only dissolved in 800 parts of boiling water; which solution did not redden paper stained with turnsole, nor the solution and tincture of this test; neither did it change turnsole paper, reddened by acid, to a blue colour. On cooling, the greatest part of what had been dissolved was deposited, in a crystallized state, equally on the sides and bottom of the vessel. This crystallized matter had the properties abovementioned (*d*.). Boiling water was found to dissolve a much greater proportion of *urinary stone*, and also of *gravel*, than of this precipitate.

3. Lye of mild potash, or subcarbonate of potash, being dropped into the solution (*e*, 2.) with its crystallized deposit, the crystals at first seemed to dissolve; but, on standing, a great part of the matter was deposited, and the liquid remained turbid.

4. The precipitate being boiled with lye of carbonate of soda, more seemed to be dissolved than in pure water; but the solution was not clear, and, on evaporating it nearly to dryness, and pouring cold water upon it, on a paper strainer, scarcely any thing but the soda passed through with the water; the precipitate remaining behind on the paper. The result was the same, when this experiment was made with a lye of carbonate of ammoniac. The result was also the same, with water in which red oxide of mercury had been boiled; which was also boiled with this precipitate, and filtrated after cooling.

5. A little of the precipitate being triturated with quick-lime, hot water was poured upon it. The filtrated liquor gave the precipitate back again, on adding muriatic acid.

6. The precipitate exposed to flame, with the blowpipe, turned black, emitted the smell of burning animal matter, and evaporated or burnt away without any signs of fusion; staining the platina spoon black.

7. Five grains of the precipitate, in half an ounce of water, were left to stand in a warm room, during the months of August and September last, without any signs of putrefaction appearing, or any obvious change taking place.

8. Twenty-four ounces of boiling water were saturated with the precipitate, and divided into six portions; from each of which, on cooling, most of it again precipitated.

The first portion, on boiling with a little lye of carbonate of

soda, (the pneumatic apparatus being affixed,) discharged no carbonic acid into lime water: but a transparent solution was produced, and, on cooling, very little was precipitated.

The second portion was, in the same manner, boiled in a little lye of caustic soda; which gave a transparent solution on cooling, without any precipitation.

The third portion being boiled with lime water, very little more seemed to be dissolved than in pure water.

The fourth portion being boiled with 4 grains of subphosphate of lime, or calcined bone, no more seemed to be dissolved on account of this addition.

Nor was more dissolved in the fifth portion, by the addition of 4 grains of phosphate of lime, made by dropping phosphoric acid into lime water.

And the result was the same with the sixth portion, to which were added 4 grains of superphosphate of lime, made by adding phosphoric acid to lime water, so as just to make a clear solution, and then evaporating the solution.

9. Urine seemed to dissolve, or at least to suspend, a greater quantity of the precipitate than mere water; so likewise did water with a little sulphate of soda.

10. The precipitate did not render solution of hard soap at all curdy; but, on adding the precipitate to solution of sulphuret of potash, it became very turbid.

11. The precipitate produced a strong effervescence, even in the cold, with nitric acid, but the fumes were not those of nitrous acid: there was a clear solution, which, on evaporation to dryness, afforded black matter, surrounded by a pink, or blood-red margin.

12. The substance, with sulphuric acid, turned black, and

emitted fumes copiously, which were scarcely those of sulphureous acid: and, on evaporation, a black mark only was left.

13. I first digested, and then boiled, in water, the precipitate with prussiate of iron; but the filtrated liquor afforded no precipitation with sulphate of iron.

14. Two drachms, by measure, of nitric acid, of the specific gravity of 1,55, were poured upon 7 grains of the precipitate. A violent effervescence took place, which was soon succeeded by a complete solution.

A few drops of this solution, being evaporated on glass, left a black mark, surrounded by a pink margin. A few drops of nitric acid being evaporated from this residue, nothing but a still less black mark, and a few red spots remained.

Nitric acid being added a third time, nothing but a black mark, still smaller, remained: which entirely disappeared, on evaporating this acid from it a fourth time.

I found that a few drops of this solution, so diluted that they did not contain the $\frac{1}{405}$, or even a much smaller part, of a grain of the precipitate, on evaporation, left a pink stain on glass.

The whole of the rest of the solution was distilled in a very low temperature, so that a drop only fell about every half-minute, till a thick brownish sediment remained, with a red margin. A similar distillation was performed, with the distilled liquor, a second time, when there remained a little whitish thick matter. On a third distillation, as before, with the distilled liquor, towards the close white fumes arose, and about half a drachm of liquid, which now remained in the retort, being left to stand, prismatical crystals, decussating each other, were formed. They had a sharp taste, but were scarcely sour;

were very soluble in the mouth, and evaporated in white fumes, leaving a very slight black stain.

15. Twenty grains of the precipitate were introduced into a tube, $\frac{1}{8}$ of an inch wide in the bore, sealed by melting at one extremity; which extremity was coated, and the tube was fitly bent for retaining sublimate, and collecting gaz. The temperature, from the fire applied, was at first very low, but was gradually increased, so as to make the coated part, containing the charge, red hot. At first, the precipitate turned black, and a little water appeared. Secondly, gaz came over, which had the smell of empyreumatic *liquor cornu cervi*. Thirdly, a brown sublimate appeared, and gaz as before, but also with prussic acid gaz. Fourthly, black matter, staining the tube, as if from tar, or animal oil. On cooling, there was found a residue, of nearly three grains, of pure carbon. The sublimate was principally carbonate of ammoniac; the rest was animal oil. The gaz discharged was nearly half its bulk, or 5 cubic inches by measure, carbonic acid; and the remaining 5 cubic inches were nitrogen gaz, containing prussic acid and empyreumatic oil.

I treated in the same manner, the same quantity of reddish crystals, deposited spontaneously from urine. The result was not very different from that of the former experiment. The gaz was more offensive, smelling like putrid urine, and the carbonaceous residue was more copious, and contained lime and phosphoric acid; at least the lixivium of it became white, on dropping into it oxalic acid; and it became slightly curdy, on adding lime water.

I treated in the same manner, some quite round and smooth

concretions, of the size of black pepper seeds. The products were the same as the former, but the gaz was still more offensive, and in smaller quantity; and the carbonaceous matter was more copious.

I, in the same way, subjected to experiment 20 grains of a nut-brown light calculus, which I had previously ascertained to contain the matter above described, which was precipitated from caustic soda by acids. The products were of the same kind as the former; but I could find no trace of phosphoric acid in the residue, which I did of lime, and the gaz was less offensive. The carbonaceous residue was not, in weight, 3 grains.

It will be proper, before I proceed further, to point out some of the more obvious conclusions from the above experiments.

1. It appears that at least one half of the matter of the urinary concretions subjected to the above experiments united to caustic soda, and was precipitated from it by acids. (II. *a*—*d*.)

2. This precipitate does not indicate acidity to the most delicate tests; (*e*, 2.) and, as it is inodorous, tasteless, (*e*, 1.) scarcely soluble in cold water, (*e*, 2.) does not unite to the alkali of carbonate of potash, of soda, or of ammoniac, (*e*, 3, 4.) nor to oxide of mercury, (*e*, 4.) nor to the lime of lime water, (*e*, 8.) nor decompound soap, (*e*, 10.) or prussiate of iron, (*e*, 13.) and, as its combination with caustic soda resembles soap, more than any double salt known to consist of an acid and alkali, this precipitate does not belong to the genus *acids*.

3. As this precipitate could not be sublimed, without being decompounded, like animal matter, (*e*, 15.) and also for the

reasons mentioned in the last paragraph, it cannot be the same thing as the *acid sublimate* of SCHEELE, or the succinic acid.

4. As it does not appear to be putrescible, nor form a viscid solution with water, it cannot be referred to the *animal mucilages.*

5. On account of its manner of burning in the air, under the blowpipe, (*e*, 6.) and its yielding, on exposure to fire in close vessels, the distinguishing products of animal matter, (especially ammoniac and prussic acid,) as well as on account of its affording a soap-like matter with caustic soda, this precipitate may be considered as a species of animal matter; and, from its composition being analogous to that of the substances called, in the new system of chemistry, *animal oxides*, it belongs to that genus. Its peculiar and specific distinguishing properties are, *imputrescibility*, *facility of crystallization*, *insolubility in cold water*, and, that most remarkable property of all others, *producing a pink or red matter, on evaporation of its solution in nitric acid.**

I do not avail myself of various other conclusions in this place, because they relate especially to the agency of medicines for preventing and removing concretions; and of course do not properly fall within the views of the Royal Society.

Having found the above precipitate to be an oxide, and not, as is commonly supposed, an acid, I thought it probable that,

* It is much to be wished that we possessed equally delicate tests of the other species of animal matter, which are confounded together, although, from their obvious properties, there is reason to believe they are of very different kinds, as is the case with the matter of the brain, liver, voluntary muscles, mucus, &c. Mr. HUNTER has discovered a distinguishing specific property of pus, and one is here indicated for the oxide of urinary concretions.

like other analogous oxides, it was *acidifiable*, and I suspected that I had really rendered it into the acid state, by the nitric acid; which, in the above experiments, (*e*, 14.) had imparted oxygen to it, and thereby rendered it soluble, deliquescent, pungent, and volatile. This change also would account for the nitric solution not affording the precipitate.

In order to obtain, for examination, an adequate quantity of this supposed acid, the following experiments were instituted, with the three acids (*viz.* the oxymuriatic, the nitro-muriatic, and the nitric,) which can acidify oxides analogous to the present one.

Experiment 1. Twenty-five grains of the above animal oxide, (for so I will now venture to call it,) and three ounces of nitric acid, of the specific gravity of 1,25, were put into a retort, and the hydro-pneumatic apparatus was adjoined.

At a very low temperature, a clear solution was made. First, soon after the solution began to boil, 23 ounces, by measure, of colourless gaz came over, which were succeeded (secondly,) by white fumes, filling the apparatus, and 23 ounces more of gaz. Thirdly, a white sublimate ascended, and there was a strong smell of prussic acid. The sublimate was very readily washed out, being very soluble, and tasted pungent or sharp, but not sour. Fourthly, the distillation being renewed, more white sublimate appeared, but only 3 ounces more of gaz came over; and then the retort only contained a dark-brown solid matter.

The first portion of gaz, *viz.* 23 ounces, consisted of about equal bulks of carbonic acid and atmospherical air. The second portion, *viz.* 23 ounces, was two-thirds of its bulk carbonic acid, and the rest nitrogen gaz. The third portion,

or 3 ounces, was atmospherical air, with a little carbonic acid.

Nitric acid was poured, in the same quantity as before, into the retort. An effervescence immediately took place, which was succeeded by a transparent solution. The distillation yielded gaz of the same kind as before, but in smaller quantity, with white fumes, and white sublimate. When only about 4 drachms, by measure, of liquid remained in the retort, a little of it was evaporated; and, when reduced to a solid matter, it turned black, and took fire, leaving a carbonaceous residue; but, before this, a margin of beautiful pink matter appeared.

Nitric acid was poured, as before, into the retort, for the third time, but very little gaz ascended, and much less white fumes than before. The distillation proceeded, till about one drachm-measure of liquid remained in the retort: this being left to stand, prismatic crystals were formed in a very small quantity of liquid. These crystals did not taste sour, but sharp, and they reddened turnsole-paper. Adding a little soda to a part of them, to see whether I could form a neutral salt, I was surprised by the extrication of ammoniac. To another portion of crystals I added sulphuric acid, which disengaged nitric acid. A third portion of crystals, being exposed over a lamp, wholly evaporated, without leaving a mark behind. The remaining matter in the retort being examined, was found to be nitrate of ammoniac. It was plain that the nitric acid had, by parting with oxygen to the carbon of the oxide, formed carbonic acid. The carbon being thus carried off, of course the nitrogen and hydrogen of the oxide uniting produce ammoniac; which, uniting with the redundant nitric acid, composes nitrate of ammoniac; but great part of the nitrate of ammoniac was carried off in the

vapour state, exhibiting white fumes, and sublimate, as above observed.

The mode of making the experiments with the other acids was of course different from the former experiment.

Experiment II. Twenty-five grains of the above animal oxide, and half an ounce of water, were put into a bottle capable of containing three pints; a stream of oxymuriatic acid gaz, from manganese and muriatic acid, was made to pass into the bottle, and upon the charge, for twelve hours; and, for twenty-four hours more, oxymuriatic gaz kept issuing, but in smaller quantity, and circulating through the bottle. The oxide, by this time, was completely dissolved. Upon adding lime to a little of the solution of it, ammoniac was disengaged; and, upon adding sulphuric acid, there was a disengagement of oxymuriatic acid. On evaporation, however, I obtained nothing but muriate of ammoniac, with which was mixed a little manganese.

In this experiment, I could not doubt that the carbon had been carried off, in the state of carbonic acid, by the oxygen of the oxymuriatic acid; and thus ammoniac was compounded, from the union of the two remaining constituent parts of the oxide, *viz.* the nitrogen and hydrogen. The oxymuriatic acid, united to the ammoniac, parted with oxygen, and became muriatic acid during evaporation; hence, muriate of ammoniac was formed.

Experiment III. The above experiment was repeated, only the gaz was nitro-muriatic gaz, from a mixture of nitric and muriatic acids. The result was the same as in the last experiment, except that the product was a mixture of nitrate, and muriate, of ammoniac.

I made other experiments of the same kind, but their results were so nearly the same as those above related, that I shall not give an account of them. By the unexpected issue of these experiments, all my hopes of acidifying the animal oxide were exploded; but I am indebted to that pursuit, for the curious discovery of the change of the most common basis of urinary concretions, (the animal oxide,) into ammoniac and carbonic acid, by the oxygen of the above acids; which will be found extremely important, as it enables us to interpret many phænomena, in a variety of cases beside the present. It now appears, that the inflammation mentioned in one of the above experiments, (and which also happened in several others,) on evaporation of the nitric solution of the animal oxide, was from the nitrate of ammoniac, the *nitrum flammans* of the old chemists, compounded in those experiments. This inflammation takes place sometimes, on evaporation of nitric solutions, both of urinary concretions, and of urine itself evaporated to the state of soft extract, on account of the ammoniac already existing in these substances. The composition of ammoniac also explains the disappearance of the whole matter of some sorts of urinary concretions, a very small residue of black matter excepted, by repeated affusion and evaporation of nitric acid, from the solution of them in this menstruum.

It remains for me to give an account of the 96 grains of powdery matter left on the paper strainer, (*a*;) which are the insoluble portion, in lye of caustic soda, of 300 grains of urinary concretions.

1. A small portion of the insoluble matter, being exposed to flame with the blowpipe, did not turn black, nor yield any

smell of animal matter; but it became whiter, and I could just agglutinate the powder into one mass, although I was unable to render it fluid.

2. The filtrated liquid, from a little of the matter boiled in water, became very turbid and white with oxalic acid: with lime water it grew barely curdy; and it did not alter the colour of turnsole, or of violet juice.

3. The matter dissolved completely in muriatic acid, and also in nitric acid, without effervescence.

This nitric solution, having been evaporated, to carry off most of the free acid, instantly became very curdy on the addition of lime water.

It grew thick and white on adding sulphuric acid, yielding a copious precipitate of sulphate of lime. One portion of the supernatant liquor upon this precipitate, on evaporation, afforded an extract-like matter; which readily melted, as phosphoric acid does when it is mixed with a little earthy matter. To the other portion of this supernatant liquor was added liquid caustic ammoniac, producing a precipitate which afforded no sulphate of magnesia with sulphuric acid.

From these experiments it appears, that the above 96 grains of insoluble matter consisted of phosphate of lime. Accordingly, the 300 grains of urinary concretions examined, appear to contain,

	grains.
Peculiar animal oxide - - - -	175
Phosphate of lime - - - -	96
Ammoniac, (and most probably phosphoric acid united to the ammoniac,) water, and common mucilage of urine, which were not collected and weighed, by estimation	29
	300

I shall next relate some experiments, made in order to obtain the acid sublimate of SCHEELE, or lithic acid of the new system of chemistry.

100 grains of an urinary concretion, which had been previously found to contain principally the above animal oxide, were introduced into a tube $\frac{1}{4}$ of an inch wide; which was sealed at one end by fusion, and which also was fitly bent for collecting sublimate, and obtaining gaz. The sealed end was coated and exposed to fire, first to a low temperature, and gradually to a very elevated one.

1. Gaz was discharged, which had the smell of burning bone.

2. Water appeared boiling immediately over the charge, which seemed to be burning, and was turned black.

3. Gaz was discharged, of the smell of empyreumatic *liquor cornu cervi*, and about half a drachm of this liquor was in the upper part of the tube.

4. A brown sublimate of carbonate of ammoniac appeared in the cold part of the tube; but in the hotter part, near the charge, was tar-like matter, and the gaz discharged had a very offensive smell of empyreumatic animal oil, with which was mixed that of prussic acid.

The coated part of the tube was kept red hot, for some time after gaz ceased to come over.

The quantity of gaz amounted to 24 ounces, by measure: it consisted of nearly 16 ounces of carbonic acid gaz, and the rest was air, with a larger proportion of nitrogen gaz than is contained in atmospheric air.

5. There was a residue of 30 grains, almost pure carbon; and 10 grains of heavy black and brown matter, a little above the coated part of the tube. In this last mentioned matter

were many small white *spicula.* At about half an inch above the carbonaceous residue, dark gray matter had been raised, which weighed 15 grains.

This sublimed gray matter did not contain any ammoniac, nor throw down any prussiate of iron, with sulphate of iron. It reddened turnsole paper and tincture. It dissolved in caustic soda; from which solution muriatic acid precipitated nothing; for, although on dropping it into the solution milkiness appeared, the liquid soon grew clear again.

Ten grains of this sublimate dissolved in four ounces of boiling water; which being evaporated to half an ounce, there was, on cooling, a copious deposit of white *spicula.** The sublimate had a sharp, but not sour taste. Being boiled in muriatic acid, and also in nitric, it did not dissolve at all; but remained, on evaporation to dryness, in the same state as before; and it must be particularly observed, that it left no red or pink matter, on evaporating the nitric acid from it. Sulphuric acid did not act upon it in the cold; but, when heated, it dissolved it, without effervescence, from which solution nothing was precipitated by caustic soda; on evaporating it to dryness, black fumes arose, leaving behind only a black stain. This sublimed matter did not render lime water turbid. Boiled in muriatic acid, so as to carry off all but a very little free acid, on the addition of lime water there was no turbid appearance, but milkiness ensued on adding oxalic acid.

The *spicula*, in the 10 grains of sublimate above mentioned, seemed to be of the same nature as the matter just described.

* From the deposition of these *spicula* by cooling, and from many of the following properties, they appear to be analogous to benzoic acid.

The whole of this sublimate amounted, by estimation, to 18 grains; and I apprehend it is the acid sublimate of SCHEELE.

The sublimate of carbonate of ammoniac amounted to 20 grains; and it was black empyreumatic animal oil which stained the tube.

This experiment was repeated, on 120 grains of a nut-brown, very light, urinary concretion. The result was not very different from that of the former experiment, except that the gaz contained a portion of hydrogen gaz. There were 30 grains of the above described *spicula*, principally mixed with carbonaceous matter: they were light, and had only a very slight sharp and bitter taste.

The experiment repeated a third time, with 80 grains of urinary concretion, afforded 15 grains of the white *spicula* above described, mixed with carbonaceous matter. These I found did dissolve in a large proportion of muriatic acid; which solution yielded them, on evaporation, in the same state as before. Under the flame applied by the blowpipe, they first melted, and then evaporated, without any smell; leaving a slight black mark. Turnsole was reddened by these *spicula*.

In a fourth experiment, I found the white *spicula* contained in the carbonaceous matter united, on boiling, with carbonate of soda, as well as with caustic soda; but, as before, muriatic acid precipitated nothing from the solution. These *spicula* could not be dissolved in nitric acid; nor did the solution of them in water become turbid with oxalic acid. Their taste was, as before, rather bitter and sharp than sour. A very suffocating smell issued forth, on breaking the tube used in this experiment, but it was not from sulphur, nor from prussic acid.

These experiments afford evidence of the wide difference between the animal oxide above described and the acid sublimate of SCHEELE.*

If this conclusion be allowed to be just, it will be necessary to give a name to this urinary animal oxide. Agreeably to the principles of the new chemical nomenclature, the name should be *Lithic oxide.* But the term *lithic* is a gross solecism; and I trust that philological critics will find the name *ouric* or *uric oxide* perfectly appropriate; for, if it be thought objectionable, on account of the existence of the matter in arthritic as well as urinary concretions, still philology will allow its admission, as in other similar cases, *κατ᾽ εξοχην*; it being found in greater abundance, by far, in the urinary passages than in other situations, and therefore falling under common observation, as an ingredient of the urine. If, however, the term lithic oxide, or any other denomination, shall obtain acceptance, I shall very willingly adopt it.

It requires no sagacity, in a person acquainted with the facts of the preceding experiments, to perceive that they are applicable to a variety of uses in chemical investigation, and in the practice of physic. The latter I of course take no notice of in this place; but, relative to the former uses, I shall particularly point out, that we are now able not only to detect, in the easiest manner, the *presence* of the minutest proportion of the above animal oxide in urinary concretions, and also in other substances, but even to determine its *proportion* to the other constituent parts,

* From these experiments, it now appears very doubtful whether the *lithic acid* of SCHEELE exists as a constituent of urinary concretions, or is compounded, in consequence of a new arrangement taking place, of the elementary matters of the concretion, by the agency of fire; but it is demonstrated, that the urinary animal oxide is really a constituent part, and even a principal one, of almost all human urinary calculi.

in the space of a few minutes, in most cases, and in all in a very little time, without any other apparatus than nitric acid, a round bottomed matrass or glass dish, and a lamp. By this method, I have, in a general way, examined above 300 specimens of concretions, of the human subject and other animals, principally urinary ones; and also many from other parts, particularly those from the joints. For these opportunities I am beholden to several professional gentlemen; whose willingness to furnish me with specimens, I shall have much satisfaction in acknowledging on a future occasion. At present, I must acknowledge my obligations to Mr. HEAVISIDE, in whose museum I found between 700 and 800 specimens. The liberal possessor of this treasure offered me, what I could not have taken the liberty of requesting, namely, permission to break off pieces from any of the articles, for experiment. Mr. EDWARD HOWARD did me the honour to take upon himself the task of writing down the reports, and otherwise assisted me.

At this time I shall only mention,

1. That out of 200 specimens of urinary calculi, not more than six did not contain the animal oxide above described, *i. e.* about 32 out of 33 contained it.

2. That the proportion of this oxide was very different; varying from $\frac{1}{200}$ (exclusive of water,) to $\frac{199}{200}$; but, for the most part, varying between $\frac{80}{200}$ and $\frac{140}{200}$.*

3. That the common animal mucilage of urine is frequently found in concretions, in very different proportions; but is perhaps never a principal constituent part of them.

* In some urinary concretions, the interior part contained this oxide, and the exterior part had none of it. On the contrary, in other urinary concretions, the exterior part contained it, and the interior part did not.

4. That the above animal oxide was not found in the urinary concretions, or any other concretions, of any animal but the human kind.

5. That this animal oxide was found also in human arthritic calculi, but not in those of the teeth, stomach, intestines, lungs, brain, *&c.*

P. S. I think proper to subjoin a few experiments, made after the preceding paper was written, which afford evidence of the truth of some of my conclusions, and enable us to explain several properties of animal concretions.

I. *On an Urinary Concretion from a Dog.*

This calculus may be said to be a great curiosity, for it is probably the only specimen in London. I owe the opportunity of examining it to Mr. H. Leigh Thomas, who met with it in the course of his dissections; and therefore we have unquestionable authority, that the concretion was really from the urinary bladder of a dog. It is worthy to be noticed, that the animal appeared to be in perfect health.

This concretion is of an oval figure; is three inches and three quarters in length, and three inches in breadth; is white as chalk; its surface is rough and uneven. Being sawed through longitudinally, no nucleus was found, nor was it laminated, but near the centre it was radiated, and contained shining *spicula*. In other parts it was, for the most part, compact and uniform in its texture. It weighed nearly ten ounces and a half. Its specific gravity was found to be greater than that of human urinary concretions, in general; which I have learned by experiments is also the case with urinary and intestinal

concretions of other brute animals, especially with those of the horse.

The specific gravity of the present calculus was 1,7.

That of one from the urinary bladder of the human subject, of the sort called mulberry calculus, and which consisted almost entirely of uric oxide, was 1,609.

That of another human urinary concretion, of the same composition as the former, but quite smooth, extracted by Mr. FORD, was 1,571.

1. The present calculus of the dog had no taste, nor smell, till exposed to fire.

2. Under the blowpipe it first became black, and emitted the smell of common animal matter; it next smelt strongly of empyreumatic *liquor cornu cervi;* and, after burning some time, became inodorous, and white, and readily melted, like super-phosphate of lime.

3. On trituration with lye of caustic soda, there was a copious discharge of ammoniac.

4. It dissolved, on boiling in nitric acid: the solution was clear and colourless; and, on evaporation to dryness, left a residue of *white bitter matter*, which, under the blowpipe emitted, weakly, the smell of animal matter.

5. Upon distilling a mixture of 150 grains of this concretion pulverized and two pints and a half of pure water, to three ounces, the distilled liquid was found to contain nothing but a little ammoniac. The three ounces of residuary liquid, being filtrated and evaporated, yielded 20 grains of phosphate of ammoniac, with a little animal matter; and the residuary undissolved matter amounted to 67 grains.

6. These 67 grains, being triturated with four ounces of caustic

soda lye, discharged very little ammoniac. On distilling this mixture to one ounce, a very small proportion only of ammoniac was found in the distilled liquid. The residuary ounce of alkaline liquid was filtrated, and mixed with the water of elutriation of the undissolved matter. One half of those liquids, on evaporation to dryness, afforded a dark brown matter, amounting to 20 grains, which consisted of phosphate of lime and animal matter. To the other half of the alkaline liquids was gradually added muriatic acid, which occasioned a deposit, in small proportion, of matter that dissolved in nitric acid, but which, on evaporation to dryness, left behind only a brownish matter, consisting of phosphate of lime and animal matter.

7. The residuary insoluble substance in caustic lye, (6.) under the blowpipe, first turned black, and then grew white, but could not be melted.

By diluted sulphuric acid it was decompounded. On the addition of nitrate of mercury, to the filtrated liquid, it yielded phosphate of mercury; and, with oxalic acid, it afforded oxalate of lime; but no sulphate of magnesia was found remaining after these precipitations were produced.

These experiments fully demonstrate, that the above concretion of a dog contained none of the uric or lithic oxide above described, but that it consisted, principally at least, of phosphate of lime, phosphate of ammoniac, and animal matter.

The present instance leads me to explain the reason of the fusibility of calculi. This is demonstrated, by the above experiments, to depend upon the discharge and decomposition of the ammoniac of the phosphate of ammoniac, during the burning away of the animal matter; hence the residuary phosphoric

acid readily fuses, and, uniting to the phosphate of lime, composes superphosphate of lime, a very fusible substance.

The phosphate of ammoniac being dissolved out by water, or caustic alkaline lye, the remaining matter is infusible, being phosphate of lime.

A very hard, brittle, and blackish intestinal calculus of a dog, from Mr. WILSON, was found to be of greater specific gravity than human urinary calculi, and to have the same composition as that of the dog above described.

This also was found to be the composition of a white, smooth, round, intestinal calculus of a horse, the specific gravity of which was 1,791.

The same composition was discovered, on examining a very hard, gray, brittle, laminated, quadrilateral concretion, said to be from the urinary bladder, but which, I think, was more probably from the intestines, of a horse.

II. *On a Calculus from the urinary Bladder of a Rabbit.*

This is also a curiosity, being the only instance I have seen. I am likewise indebted to Mr. THOMAS for this specimen, which he very kindly sent me, fitted up as a preparation, included in the bladder itself. Mr. THOMAS found this concretion, on dissecting a perfectly healthy and very fat rabbit.

This specimen is spherical, and of the size of a small nutmeg. It is of a dark brown colour, has a smooth surface, is hard, brittle, and heavy. When broken, it appeared to consist of concentric laminæ. Its specific gravity was 2.

1. Under the blowpipe it grew black, and emitted the smell of animal matter while burning; at last it ceased to emit any

smell; and, urged with the intensest fire, showed no signs of fusibility.

2. It readily dissolved, with effervescence, like marble, in both muriatic and nitric acids, giving clear solutions.

3. The nitric solution (2.) being evaporated partly to dryness, and partly to the consistence of extract, the dry residuary matter was white; and the extract-like matter, which was bitter, could not be fused under the blowpipe; but, when brought to the state of a powder, the particles of it were made to cohere loosely together into one mass.

4. On dropping sulphuric acid into the muriatic solution, (2.) turbidness, and a copious white precipitation, immediately ensued, from the composition of sulphate of lime.

From these experiments it is warrantable to conclude, that the above urinary calculus of a rabbit consisted principally of carbonate of lime and common animal matter, with, perhaps, a very small proportion of phosphoric acid: it certainly contained no uric oxide.

I examined, in the same manner, a concretion which was said to be from the stomach of a monkey; but I have not evidence of its origin equally satisfactory as that of the two last calculi. Its composition was found to be similar to that of the calculus of the rabbit, *viz.* carbonate of lime and animal matter. Its obvious properties were also the same; it was of the size of the largest nutmeg.

III. *On urinary Concretions of the Horse.*

I examined several specimens in cabinets, said to be vesical calculi of the horse, and found none of them to contain the uric oxide above described; but that they consisted (as well

as the calculi from the stomach and intestines of the same animal) of phosphate of lime, phosphate of ammoniac, and common animal matter, which melted like superphosphate of lime, after burning away the animal matter and ammoniac. As these, and some other experiments, seemed to concur in establishing an important truth, I thought it necessary to examine an urinary concretion of a horse, which, from its figure and size, was unquestionably from the kidney of that animal; for I have found by experience, that one cannot depend entirely on the accounts in cabinets, nor indeed, sometimes, on the assertions of persons who collect specimens.

1. This concretion, which Dr. BAILLIE was so good as to give me, was of a blackish colour, was very brittle and hard, and had no smell or taste. It felt heavier than human urinary calculi.

2. Under the blowpipe it became quite black, and emitted the smell, weakly, of common animal matter. It was reduced very little in quantity, and showed no appearances of fusibility, after being exposed for a considerable time to the most intense fire of the blowpipe.

3. Muriatic acid dissolved this concretion, with effervescence, yielding a clear solution; which, on evaporation to dryness, left a black and bitter residue.

4. A little of the residue (3.) being boiled in pure water, to the filtrated liquor superoxalate of potash was added; which occasioned a very turbid appearance, and copious white precipitation.

5. Nitric acid also readily dissolved this concretion, with effervescence. The solution being evaporated, partly to dryness, and partly to the consistence of an extract, the dry residuary

matter was white and bitterish, and the extract-like part showed no signs of fusibility under the intensest fire of the blowpipe.

6. A little of the concretion, being triturated with lye of caustic soda, emitted no smell of ammoniac.

From these experiments it appears, that this calculus, like the former one from a rabbit, consists of carbonate of lime and common animal matter.

A renal calculus of a horse, in Mr. HEAVISIDE's collection, appeared, on examination, to consist of carbonate of lime and common animal matter.

Another specimen, however, of renal calculus of a horse, in the same collection, marked No. 3. was found to consist of phosphate of lime, phosphate of ammoniac, and common animal matter. It was fused under the blowpipe.

The specimen marked No. 8. in the same collection, which was said to be a vesical calculus of a horse, appeared to consist of the three ingredients just mentioned.

I have met with two instances of a deposit of a prodigious quantity of matter in the urinary bladder of horses, which had not crystallized, or even concreted: it amounted, in one specimen, which was given to me by Dr. MARSHALL, to several pounds weight; and in the other, which is in the possession of Mr. HOME, to about 45 pounds. Its composition was, principally, carbonate of lime and common animal matter.*

I have not found any instance of human urinary calculi of a

* Since this paper was read, Mr. BLIZARD has been so attentive as to send me another specimen of the same kind of deposit as those here mentioned. It now appears probable, that such deposits frequently take place, although I believe they have not been noticed before.

similar composition to that of the rabbit, and those of horses above described, which consist of carbonate of lime and animal matter; and I believe that human urinary calculi very rarely occur of a similar composition to those of the dog and horses above mentioned, which were found to consist of phosphate of ammoniac, phosphate of lime, and animal matter, without containing *uric oxide*.

The difference in the constitution of urinary concretions may depend on the difference of the urinary organs of different animals, on the food and drink,* and on the various diseased and healthy states of the urinary organs.

I have not found the uric oxide in the urinary concretions of any phytivorous animal; but, whether it would be formed in the human animal when nourished merely by vegetable matter, must be determined by future observations. In the mean time, it is warrantable to conclude, from analogy, that it would not, and the application of this fact to practice is obvious; but I now purposely avoid making any practical inferences, until I can, at the same time, state a number of facts I have collected, relative both to concretions and to the urine itself.

* I found the stomach-concretion called *Oriental Bezoar*, to consist merely of vegetable matter; as did the intestinal concretion of a sheep.

III. *On the Discovery of four additional Satellites of the* Georgium Sidus. *The retrograde Motion of its old Satellites announced; and the Cause of their Disappearance at certain Distances from the Planet explained. By* William Herschel, *LL.D. F.R.S.*

Read December 14, 1797.

HAVING been lately much engaged in improving my tables for calculating the places of the Georgian satellites, I found it necessary to recompute all my observations of them. In looking over the whole series, from the year of the first discovery of the satellites in 1787 to the present time, I found these observations so extensive, especially with regard to a miscellaneous branch of them, that I resolved to make this latter part the subject of a strict examination.

The observations I allude to relate to the discovery of four additional satellites: to surmises of a large and a small ring, at rectangles to each other: to the light and size of the satellites: and to their disappearance at certain distances from the planet.

In this undertaking, I was much assisted by a set of short and easy theorems I had laid down for calculating all the particulars respecting the motions of satellites; such as, finding the longitude of the satellite from the angle of position, or the position from the longitude: the inclination of the orbit from the angle of position and longitude: the apogee: the greatest

elongation; and other particulars. Having moreover calculated tables for reduction: for the position of the point of greatest elongation; and for the distance of the apogee, or opening of the ellipsis; and also contrived an expeditious application of the globe for checking computations of this sort, I found many former intricacies vanish.

By the help of these tables and theorems, I could examine the miscellaneous observations relating to additional satellites, on a supposition that their orbits were in the same plane with the two already known, and that the direction of their motion was also the same with that of the latter.

And here I take an opportunity to announce, that the motion of the Georgian satellites is retrograde.

This seems to be a remarkable instance of the great variety that takes place among the movements of the heavenly bodies. Hitherto, all the planets and satellites of the solar system have been found to direct their course according to the order of the signs: even the diurnal or rotatory motions, not only of the primary planets, but also of the sun, and six of their secondaries or satellites, now are known to follow the same direction; but here we have two considerable celestial bodies completing their revolutions in a retrograde order.

I return to the examination of the miscellaneous observations, the result of which has been of considerable importance, and will be contained in this paper. The existence of four additional satellites of our new planet will be proved. The observations which tend to ascertain the existence of rings not appearing to be satisfactorily supported, it will be proper that surmises of them should either be given up, as ill founded, or at least reserved till superior instruments can be provided, to

throw more light upon the subject. A remarkable phænomenon, of the vanishing of the satellites, will be shewn to take place, and its cause animadverted upon.

I shall now, in the first place, relate the observations on which these conclusions must rest for support, and afterwards join some short arguments, to shew that my results are fairly deduced from them.

For the sake of perspicuity, I shall arrange the observations under three different heads; and begin with those which relate to the discovery of additional satellites.

A great number of observations on supposed satellites, that were afterwards found to be stars, or of which it could not be ascertained whether they were stars or satellites, for want of clear weather, will only be related. For, to enter into the particular manner of recording these supposed satellites, or to give the figures which were delineated to point them out, would take up too much time, and be of no considerable service to our present argument. It ought however to be mentioned, that nearly the same precaution was taken with all the related observations as, it will be found, was used in those that are given in the words of the journals that contain them. The former will be distinguished under the head *Reports*, the latter under that of *Observations.*

Investigation of additional Satellites.

Reports.

Feb. 6, 1782. A very faint star was pointed out as probably a satellite, but Feb. 7 and 8 was found remaining in its former situation.

March 4, 1783. A satellite was suspected, but March 8 was found to be a star.

April 5, 1783. A suspected satellite was delineated, but the 6th it was seen remaining in its former place.

Nov. 19, 1783. A supposed satellite was marked down, but no opportunity could be had to account for it afterwards.

Nov. 16, 1784. Supposed 1st and 2d satellites were pointed out, but not accounted for afterwards.

Many other fruitless endeavours for the discovery of satellites were made; but, finding my instrument, in the NEWTONIAN form, not adequate to the undertaking, the pursuit was partly relinquished. The additional light however which I gained, by introducing the Front-view in my telescope, soon after gave me an opportunity of resuming it with more success.

Jan. 11, 1787. Three supposed satellites were observed: a first, a second, and a third. Jan. 12, the 1st and 2d were gone from the places in which I had marked them, but the 3d was remaining, and therefore was a fixed star.*

Jan. 14. A supposed 3d satellite was delineated, but on the 17th it was found to be a star.

Jan. 17. Supposed 3d, 4th, and 5th satellites were marked, but were found remaining in their former places on the 18th.

Jan. 24. Supposed 3d and 4th satellites were noted, but the weather proving bad on the succeeding nights, till February 4, they were lost in uncertainty.

Feb. 4. A 3d satellite was marked, but not being afterwards accounted for remains lost.

Feb. 7. A supposed 3d satellite was proved to be a star the 9th.

Feb. 10. Supposed 3d and 4th satellites have not been afterwards accounted for.

* It has already been shewn, in a former paper, that the removed satellites were those two which now are sufficiently known.

Feb. 13. Supposed 3d, 4th, and 5th satellites proved stars the 16th.

Feb. 16. A 3d satellite proved a star the 17th.

Feb. 19. Supposed 3d and 4th satellites were proved to be stars the same evening, by being left in their places, while the planet was moving on.

Feb. 22. The supposed 3d and 4th of the 19th were seen remaining in their former places; and new 3d, 4th, and 5th satellites were marked; but these were lost through bad weather, which lasted till March 4.

March 5. A supposed 3d satellite proved to be a star the 7th.

March 7. The position of a 3d was taken, and a 4th also marked; but March 8 they were both proved to be fixed stars.

October 20. A very small star was seen near the planet, but lost, for want of opportunity to account for it.

March 13, 1789. The positions of 3d and 4th satellites were taken, but the 14th they were found to be stars.

March 16. Supposed 3d and 4th satellites were well laid down, but March 20 were found to be stars.

March 26. The places of supposed 3d and 4th satellites were ascertained, but no opportunity could be had of deciding whether they were stars or satellites.

Dec. 15. A supposed 3d satellite was accurately delineated, but proved to be a star the 16th.

Observations.

"Jan. 18, 1790. $6^h\ 51'$.* A supposed 3d satellite is about

* All the times have been corrected so as to be true, sidereal; but are only given here to the nearest minute.

" 2 diameters of the planet following; excessively faint, and " only seen by glimpses."

" 7^h $57'$. I cannot perceive the 3d."

Reports.

Jan. 18, 1790. A supposed 4th satellite was described, but was found to be a star the 19th.

Jan. 20. A 3d satellite was perceived, and its angle of position ascertained; but was afterwards lost, for want of opportunity to examine its place again.

Observations.

" Feb. 9, 1790. 6^h $28'$. There is a supposed 3d satellite, in a " line with the planet and the 2d satellite."

" 6^h $40'$. Configuration of the Georgian planet and satel- " lites." See Tab. II. fig. 1.

" Clouds prevent further observations."

" Feb. 11. The supposed 3d satellite of the 9th of February " I believe is wanting; at least I cannot see it, though the wea- " ther is very clear, but windy."

" Feb. 12. The supposed 3d satellite of the 9th is not in " the place where I saw it that night."

Reports.

Feb. 11, 1790. Supposed 3d and 4th satellites were laid down, but on the 12th they were both found remaining in their former places.

Feb. 16. A 3d satellite was delineated, but on the 17th it proved to be a star.

March 5. Supposed 3d and 4th satellites were laid down, but on the 8th were seen remaining in their places.

Feb. 4, 1791. A 3d satellite was marked, but has not been accounted for afterwards.

Feb. 5. Supposed 3d, 4th, and 5th satellites were delineated, but no opportunity could afterwards be found to ascertain their existence.

March 5. Supposed 3d, 4th, and 5th satellites were put down. They could not be seen March 6, but were proved to be small stars the 7th.

Feb. 12, 1792. A third satellite was delineated, but was left behind by the planet the same evening, and also seen in its former place the next night.

Feb. 13. A 3d satellite was put down, but proved to be a star the 14th.

Feb. 20. The position of a 3d satellite was taken, but 4 hours after was found to be left behind by the planet. It was also seen in its former place Feb. 21.

Feb. 26. A 3d satellite, between the planet and 2d, was observed; which, $3^h\ 37'$ afterwards, was thought to be left behind, but was so faint as hardly to be perceivable. A fourth was also put down. Neither of them have been accounted for afterwards.

March 8, 1793. The position of a supposed 3d satellite was taken, but the next day it was found to be a star.

March 9. A supposed 3d satellite was observed, at 5 or 6 times the distance of the 1st, but was not accounted for afterwards.

March 14. Supposed 3d and 4th satellites were observed, but no opportunity could be had afterwards to see them again.

Observations.

"Feb. 25, 1794. With 320, there is a small star *a*, fig. 2. "about 15 degrees north preceding the planet; and another *b*, "about 30 degrees north preceding: also one *c*, directly pre-"ceding. There is a very small fourth star *d*, making a trape-"zium with the other three; and two more *e f*, preceding this "4th star, are in a line with it."

"Feb. 26. The stars, in figure 2, marked *f e d a*, are in a line. "There is a star *g*, at rectangles to *f e d a*: the perpendicular "falls upon *d*: it is towards the south. There is also a star *b*, "north of *f e d a*; but it is too faint to admit of a determination "of its place: I can only see it now and then by imperfect "glimpses."

"Feb. 28. $6^h\ 40'$. The stars *f e d a* of the 26th are in their "places. *c* is in the place where I have marked it. The star "*g* is in the place where I marked it. I see also the very "small star *b*."

"$6^h\ 50'$. There is a very small star *k*, but not so small as *b*, "very near to, and north following *f*, which I did not see on "the 26th. It is not quite half way between *f* and *e*, but "nearer to *f* than to *e*. It makes an obtuse triangle with *f* "and *e*."

"$9^h\ 43'$. The motion of the planet this evening, since the "first observation, is very visible."

"$10^h\ 7'$. I cannot perceive the star *k*. The weather is not "so clear as it was."

"$10^h\ 21'$. I cannot perceive the star *k* in the place where it "was $6^h\ 50'$."

"March 4, 1794. Power 320. $6^h\ 46'$. The stars *a b c d e f g*

" of Feb. 28, fig. 3. are in their places, but I cannot see the " small star *k*. The evening is not very clear."

" 9^h 51′. I cannot see the star *k*."

" 10^h 25′. I suppose *a*, in figure 4, to be the star towards " which the planet is moving."

" *c a b* are in a crooked line.

" *c e f* are nearly in a line; *f* is a little preceding.

" *c d e* form a triangle.

" There is a small star *h*, preceding *d*.

" There is an exceeding small star *k*, in the line *h k g*, but a " little preceding and nearer *h*.

" *a b c* are large stars.

" *d e g* are also pretty large.

" *f* and *h* are small. Power 157.

" With 320, there is also a very small star *l*, near *d*, forming " an isosceles triangle *h d l*, on the preceding side."

" March 5. 7^h 39′. Power 320. The stars *a b c d e f g h k l* " are in the places where they were marked last night."

" 9^h 37′. There is a very small star *n*, south of *g*; another *m*, " preceding *g*; and a third *o*, south following *g*."

" 10^h 19′. I suspect a very small star, south following the " planet, at one-third of the distance of the 1st satellite; but " cannot verify it with 480. With 600, the same suspicion " continues."

" March 7. 9^h 48′. The stars *a b c d e f g h k l* are in their " places."

" *n m o* are in their places."

" The planet has passed between the stars *e f*, pretty near " to *f*."

Reports.

March 21, 1794. Power 320. A small star was suspected south of the planet, or about 85° south following. It could not be verified with 480, nor with 600; and was even supposed to have been a deception; but the 22d was found remaining in the place where the planet had left it.

Observations.

"March 26, 1794. $9^h\,35'$. With 480, I see the 1st satellite "much better than with 320. I suspected, with 320, a 3d sa-"tellite, directly north of the planet, a little farther off than the "1st, and this power almost verifies the suspicion." See figure 5. (Tab. III.)

"$9^h\,44'$. With 600, I still suspect the same, but cannot sa-"tisfy myself of the reality."

"$11^h\,32'$. I see the supposed 3d satellite perfectly well now. "It is much smaller than the 1st, and in a line with the planet "and the 1st; so that probably it is a fixed star; since it pre-"ceded the 1st, when I saw it before, I think more than the "quicker motion of the 1st satellite would account for. If it "be a fixed star, it makes almost a rectangular triangle with *q r*, "the shorter leg being 3d *r*; or it is almost in a line with *q* "and *n*."

"*N. B.* The lines in the description are truer than in the "figure, as the latter is only intended to point out the stars "in question."

"March 27. $8^h\,37'$. Power 320. The same small star, ob-"served last night at $11^h\,32'$, is gone from the place where I "saw it. From its light last night, compared to *r*, which

"to-night is very near the planet, and scarcely visible, I am "certain that it must be bright enough to be perceived imme- "diately, if it were in the place pointed out by my descrip- "tion."

"$10^h\ 20'$. The planet is considerably removed from the "star *r*.

"$11^h\ 41'$. I had many glimpses of small stars or supposed "satellites: one of them in a place agreeing with the 3d satel- "lite of last night, (supposing it to have moved with the planet;) "that is, a little farther off, and after the 1st. Another pre- "ceding the 1st, but nearer. Some others south, at a good dis- "tance; but not one of them could I see for any constancy. "They were only lucid glimpses."

Reports.

March 27, 1794. A supposed 4th satellite was delineated, but proved to be a star the 28th.

Observations.

"March 4, 1796. Configuration of the Georgian planet and "fixed stars for $10^h\ 3'$." See fig. 6.

"March 5. $9^h\ 50'$. I suspected a very small star between *c* "and *b*, which was not there last night. I had a pretty cer- "tain glimpse of it. It is in a line from the planet towards *f*: "power 320. With 600, I see the satellite better than before; "but cannot perceive the suspected small star."

"$10^h\ 17'$. The air is remarkably clear at present, but I can- "not perceive the suspected star."

"March 9. $11^h\ 23'$. As the probability of other satellites is, "that they revolve in the same plane with the 1st and 2d, I

"chiefly look for them in the direction of their orbits, which "is now nearly a straight line."

"April 5, 1796. There is no star in the line of the transverse, "that can be taken for a satellite: the evening is very beauti- "ful, and I examined that line with 900, at a distance; and "with 600, within the orbits of the two satellites."

"March 23, 1797. Three very small stars O P Q, are in the "path of the planet; they form an obtuse triangle."

"March 25. $11^h\ 4'$. A very bright star S, at almost the dis- "tance of the field of view, is a little south of the path of the "planet. It has a small north preceding star T, which points "to two more V W, towards the north."

"Between the triangle of March 23d and the four last men- "tioned stars, is a very small star X."

"March 28. $10^h\ 52'$. I see the stars S T V W X of March "25th."

"$11^h\ 25'$. From X towards the triangle O P Q of March 23d, "is an exceeding small star Y, about four times the distance "of the 2d satellite, and nearly in the line of the greatest elonga- "tion. I do not remember to have seen it the 25th."

"$11^h\ 41'$. The distance of Y from X is about $\frac{1}{4}$ of the dis- "tance of X from the triangle. It requires much attention to "see it; but I have a very complete view of it, by drawing "the planet just out of the field, and the star X almost on "the preceding side."

Arguments upon the Reports and Observations.

From the reports of the great number of supposed satellites, compared with the select observations which are given at length, it must be evident that the method of looking for difficult

objects, and of marking them down by lines and angles, with every other possible advantage for finding them again, has been completely understood and put in practice. So guarded against deceptions, we cannot but allow, that even a single glimpse of a very small star is a considerable argument in favour of its existence. What I call verifying a suspicion, which is generally done with a higher power than that which caused the suspicion, is obtaining a steadier view of the existence of the object in question; that is, to see it in such a manner as to be able to fix an eye upon it, and to compare it with other surrounding objects; and thus to be able to ascertain its relative situation with those other objects, in a satisfactory manner.

An interior Satellite.

The observation of Jan. 18, 1790, says, "a supposed 3d satellite is about two diameters of the planet following."* There is not the least doubt expressed about the existence of the satellite, or object in question, which therefore must be looked upon as ascertained. Now, the angle of the greatest elongation of the Georgian satellites, by my new tables, at the time of observation, was 81°33′ N.F. Therefore, the angle of the apogee was 8°27′ S.F.; and since, by observation, the satellite was "following," without any mention of degrees being made, we may admit it to have been not far from the parallel; suppose 11 or 12 degrees S.F. In this case, the satellite would be in the apogee about the time of the 2d observation, at $7^h\,57'$; which says, "I cannot perceive the satellite." But it will be shewn hereafter, when I come to treat of the vanishing of the satellites, that it would become invisible in this situation. Indeed, without the supposition of

the satellite's coming to the apogee, it might easily happen that the least change in the clearness of the air, during a time of $1^h\ 5'$, which elapsed between the first and second observation, might render an object invisible, which, as the first observation says, was "excessively faint, and could only be seen by "glimpses."

From the observed distance, which is put at "2 diameters "of the planet," we may conclude what would be the distance of its greatest elongation. For, 2 diameters from the disk of the planet give $2\frac{1}{2}$ from the centre. Now, the distance of the apogee at this time, by my tables, was ,64, supposing that of the greatest elongation 1; therefore we have the radius of its orbit $\frac{2,5 \times 4'',12}{,64} = 16'',1$.

This calculation is not intended to determine precisely the distance of the satellite, but only to shew that its orbit is more contracted than that of the 1st, and that consequently it is an interior satellite.

If any doubt should be entertained about the validity of this observation, we have a second, and very striking one, of March 5, 1794; where an interior satellite was suspected south following the planet, at one-third of the distance of the 1st. March 4, when a description was made of the stars, as in figure 4, this satellite was not in the place where it was observed the 5th. And, by an examination of the same stars March 7, it appears, that even the smallest stars *n m o*, of the 5th, were seen in their former places, but not the satellite. The observation therefore must be looked upon as decisive with regard to its existence. If any doubt should arise, on account of the suspicion not being verified with 480, I must remark, that being used to such imperfect glimpses, it has generally

turned out, even when I have given up as improbable the existence of a supposed satellite seen in that manner, that it has afterwards nevertheless been discovered that a small star remained in the place where the satellite had been suspected to be situated. An instance of this may be seen in the report of the observations that were made March 21 and 22, 1794. Besides, in the present case, it is additionally mentioned, that the same object was examined with a power of 600, which continued the suspicion.

From the assigned place of this satellite, at $\frac{1}{3}$ of the distance of that of the first, it appears that this observation belongs to the interior satellite of Jan. 18, 1790, which has already been examined. The 1st satellite was this evening at its greatest elongation, one-third of which is about 11″. The apogee distance of a satellite whose greatest distance is 16″,1 would have been 6″,1 on the day of our observation; but, not being come to the apogee, by many degrees, it could not be so near the planet.

For the sake of greater precision, let us admit that the satellite was exactly south following; that is, 45 degrees from the parallel, and 45 from the meridian; then, by calculation, a satellite whose orbit is at 16″,1 from the planet, would, in the situation now admitted, have been 7″,1 from its centre, which might coarsely be rated at $\frac{1}{3}$ of the distance of the first. But the estimation of 11″ is probably more accurate than that in the 1st observation, where 2 diameters are given. And, by calculating from this quantity, we find that the greatest elongation distance of the satellite is 25″,5; now, putting $2\frac{1}{2}$ diameters in the first observation, instead of 2, the distance deduced from it will come out 19″,3; which is certainly an agreement

sufficiently near to admit both observations to belong to the same satellite.

March 27, 1794. We find a third observation, which will assist in supporting the two former ones. A glimpse of a satellite is mentioned, which was preceding the 1st, but nearer the planet. The position of the 1st satellite the same evening was, by measuring, found to be 62°,1 N.F. which is still a considerable way from its greatest elongation; but our new satellite preceded it, and was therefore more advanced in its orbit, or nearer its greatest distance; and yet the observation says, that it was not so far from the planet as the 1st; notwithstanding this latter was in a more contracted part of its orbit. It follows therefore that this was also an interior satellite. Now, since we may allow these three observations to belong to the same, we ought not to make a distinction; but admit, as sufficiently established, the existence of at least one interior satellite of our new planet.

An intermediate Satellite.

March 26, 1794. A satellite was suspected, directly north of the planet. At first it could not be verified, but was seen perfectly well afterwards. It was supposed that probably it might be a star, but this was left undecided. The observation of March 27th however removes all doubt upon the subject; as it fully affirms that the small star observed the 26th, at $11^h\ 32'$, was gone from the place in which it was the day before. Such strong circumstances are mentioned in confirmation, that we cannot hesitate placing this among the list of existing satellites. It was not the interior satellite of Jan. 18, 1790; for both the

1st and 2d known satellites were in full view March 26th; see figure 5. and the observation places this new one in a line drawn from the planet continued through the 1st; with the remark, that it was a little farther from the planet than the 1st. The 2d was then near its greatest southern elongation, and we may see from the figure, as well as from the above description, that the orbit of this new satellite is situated between the orbits of the other two.

We have a second observation of the same satellite March 27, 1794: where, among the glimpses of additional satellites at $11^h\ 41'$, is mentioned "one in a place probably agreeing with "the new satellite of March 26th," which, by its motion, must have been carried forward, so as to be where the observation of the 27th says it was, namely, "a little farther off and after the "1st;" that is, at a little greater distance from the planet than the 1st, and not so far advanced in its orbit as that satellite. This amounts not only to an additional proof, but even announces the recognition of the satellite, and its motion in the course of one day.

An exterior Satellite.

Feb. 9, 1790. A new satellite was seen, in a line with the planet and the 2d satellite. See figure 1. To convince us that this was not a fixed star, we have the observations of two other nights, the 11th and 12th of February, where the removal of it, from the place in which it was Feb. 9, is clearly demonstrated. As it was in a line continued from the planet through the second satellite, its orbit must evidently be of a greater dimension than that of the 2d; I shall therefore put it down as an exterior satellite.

Most likely this satellite also was seen among the supposed satellites south of the planet, March 27, 1794; where we find mention made of "some others south, at a good distance." In that case, this will make a second observation.

We have a third observation of the same new satellite March 5, 1796; when a very small star was seen, in a place where the evening before there had been none; as appears by the configuration of the 5th of March. See figure 6. At the time of the observation, the planet was come to the longitude of the place where the star was perceived to be; which agrees with the idea of its having been brought to that situation by the planet. It may be objected, that the star could not be verified with a power of 600; but here we have more than a bare suspicion of the satellite, for the observation says, "I had a pretty *certain* "glimpse of it;" and this appears also from the assigned place of the star at the intersection of two given lines. For, such a delineation could not have been made, without having perceived it with a considerable degree of steady vision. Its distance, to judge by the description, will agree sufficiently with the two foregoing observations of this new exterior satellite.

The most distant Satellite.

On Feb. 28, 1794, a star was perceived where on the 26th there was none. This star was larger than a very small star which was observed the 26th, not far from the place of the new supposed satellite; and a configuration having been made expressly, by way of ascertaining what stars might afterwards come into a situation where they could be mistaken for satellites, our new star or satellite would not have been omitted,

when a smaller one very near it was scrupulously recorded. The motion of the planet, in 3 hours and 3 minutes, is mentioned as very visible. The place of the star, which was a new visitor this evening, was very particularly delineated, at $6^h\ 50'$. From its situation, it is evident the motion of the planet must have carried this star, if it was one of its satellites, towards the large star *f*, figure 3; in the light of which a dim satellite would be lost. This accordingly happened; for at $10^h\ 7'$ and $10^h\ 21'$ it was no longer visible. The direction of the planet's motion is plainly pointed out, by the place of the planet March 2d.

With respect to the orbit of this satellite, it appears, from its situation near the apogee, where it was seen, that its distance was to that of the second satellite, which was then near its greatest elongation, as 8 to 5. And, since the apogee distance, on the day of observation, was only ,37, we have its greatest elongation as $\frac{8}{,37}$ to 5; that is, as 21,6 to 5, or above 4 to 1. From which we may conclude, that its orbit must lie considerably without the before mentioned exterior satellite of Feb. 9, 1790.

We have a second observation of it March 27, 1794; which, though not very strong, yet adds confirmation to the former. For that evening, which was uncommonly fine, other satellites, south, at a good distance, were perceived. This must relate principally to our present satellite, which may certainly be said to be at a good distance from the planet, and which, by that time, was probably in the southern part of its orbit, and near its greatest elongation.

There is a third observation, March 28, 1797, which probably also belongs to this satellite. For the exceedingly small star Y,

which is mentioned as not having been seen the 25th, when the delineation of the stars was made, will agree very well with the two former observations; and, being near the greatest elongation, the distance of this satellite is well pointed out, and agrees remarkably well with the calculation of the first observation of it.

It remains now only to be mentioned, that in such delicate observations as these of the additional satellites, there may possibly arise some doubts with those who are very scrupulous; but, as I have been much in the habit of seeing very small and dim objects, I have not been detained from publishing these observations sooner, on account of the least uncertainty about the existence of these satellites, but merely because I was in hopes of being able soon to give a better account of them, with regard to their periodical revolutions. It did not appear satisfactory to me to announce a satellite, unless I could, at the same time, have pointed out more precisely the place where it might be found by other astronomers. But, as more time is now already elapsed than I had allowed myself for a completion of the theory of these satellites, I thought it better not to defer the communication any longer.

The arrangement of the four new and the two old satellites together will be thus:

First satellite, the interior one of Jan. 18, 1790.
Second satellite, the nearest old one of Jan. 11, 1787.
Third satellite, the intermediate one of March 26, 1794.
Fourth satellite, the farthest old one of Jan. 11, 1787.
Fifth satellite, the exterior one of Feb. 9, 1790.
Sixth satellite, the most distant one of Feb. 28, 1794.

*Observations and Reports tending to the Discovery of one or more Rings of the Georgian Planet, and the flattening of its polar Regions.**

"Nov. 13, 1782. 7-feet reflector, power 460. I perceive no "flattening of the polar regions."

"April 8, 1783. I surmise a polar flattening."

"Feb. 4, 1787. 20-feet reflector, power 300. Well defined; "no appearance of any ring; much daylight."

"March 4. I begin to entertain again a suspicion that the "planet is not round. When I see it most distinctly, it appears "to have double, opposite points. See figure 7. Perhaps a "double ring; that is, two rings, at rectangles to each other."

March 5. The Georgian Sidus not being round, the telescope was turned to Jupiter. I viewed that planet with 157, 300, and 480, which shewed it perfectly well defined. Returning to the Georgian planet, it was again seen affected with projecting points. Two opposite ones, that were large and blunt, from preceding to following; and two others, that were small and less blunt, from north to south. See figure 7.

March 7. Position of the great ring R, from 70° S.P. to 70° N.F. Small ring *r*, from 20° N.P. to 20° N.F. 600 shewed R and *r*. 800 R and *r*. 1200 R and *r*.

"March 8. R and *r* are probably deceptions."

"Nov. 9. The suspicion of a ring returns often when I ad-"just the focus by one of the satellites, but yet I think it has "no foundation."

Feb. 22, 1789. A ring was suspected.

* The observations are distinguished from the reports by marks of quotation, (" ".)

"March 16. $7^h\ 37'$. I have turned my speculum 90° round.
"A certain appearance, owing to a defect which it has con-
"tracted by exposure to the air since it was made, is gone
"with it; (see fig. 9 and 10;) but the suspected ring remains
in the place where I saw it last.

"$7^h\ 50'$. Power 471 shews the same appearance rather
"stronger. Power 589 still shews the same."

"*Memorandum.* The ring is short, not like that of Saturn.
"It seems to be as in figure 8; and this may account for the
"great difficulty of verifying it. It is remarkable that the two
"*ansæ* seem of a colour a little inclined to red. The blur oc-
"casioned by the fault of the speculum is, to-night, as repre-
"sented in figure 9. The other evening it was as in figure 10;
"and the ring is likewise as it was the same evening."

"March 20. $7^h\ 53'$. When the satellites are best in focus,
"the suspicion of a ring is the strongest."

"Dec. 15. The planet is not round, and I have not much
"doubt but that it has a ring."

"Feb. 26, 1792. $6^h\ 34'$. My telescope is extremely distinct;
"and, when I adjust it upon a very minute double star, which
"is not far from the planet, I see a very faint ray, like a ring
"crossing the planet, over the centre. This appearance is of
"an equal length on both sides, so that I strongly suspect it
"to be a ring. There is, however, a possibility of its being an
"imperfection in the speculum, owing to some slight scratch:
"I shall take its position, and afterwards turn the speculum on
"its axis."

"$8^h\ 39'$. Position of the supposed ring 55°,6 from N.P. to S.F."

"$9^h\ 56'$. I have turned the speculum one quadrant round;
"but the appearance of the very faint ray continues where it

"was before, so that the defect is not in the speculum, nor is "it in the eye-glass. But still it is now also pretty evident "that it arises from some external cause; for it is now in the "same situation, with regard to the tube, in which it was $3\frac{1}{2}$ "hours ago: whereas, the parallel is differently situated, and "the ring, of course, ought to be so too."

"March 5, 1792. I viewed the Georgian planet with a newly "polished speculum, of an excellent figure. It shewed the pla-"net very well defined, and without any suspicion of a ring. "I viewed it successively with 240, 300, 480, 600, 800, 1200, "and 2400; all which powers my speculum bore with great dis-"tinctness. I am pretty well convinced that the disk is flat-"tened." The moon was pretty near the planet.

"Dec. 4, 1793. 7-feet reflector, power 287. The Georgian "planet is not so well defined as, from the extraordinary dis-"tinctness of my present 7-feet telescope, it ought to be. There "is a suspicion of some apparatus about the planet."

"Feb. 26, 1794. 20-feet reflector, power 480. The planet "seems to be a little lengthened out, in the direction of the "longer axis of the satellites' orbits."

"April 21, 1795. 10-feet reflector, power 400. The telescope "adjusted to a neighbouring star, so as to make it perfectly "round. The disk of the planet seems to be a little elliptical. "With 600, also adjusted upon the neighbouring star, the disk "still seems elliptical."

Remarks upon the foregoing Observations.

With regard to the phænomena which gave rise to the suspicion of one or more rings, it must be noticed, that few specula or object-glasses are so very perfect as not to be affected

with some rays or inequalities, when high powers are used, and the object to be viewed is very minute. It seems, however, from the observations of March 16, 1789, and Feb. 26, 1792, that the cause of deception, in this case, must be looked for elsewhere. It has often happened, that the situation of the eye-glass, being on one side of the tube, which brings the observer close to the mouth of it, has occasioned a visible defect in the view of a very minute object, when proper care has not been taken to keep out of the way; especially when the wind is in such a quarter as to come from the observer across the telescope. The direction of a current of air alone may also affect vision. Without, however, entering further into the discussion of a subject that must be attended with uncertainty, I will only add, that the observation of the 26th seems to be very decisive against the existence of a ring. When the surmises arose at first, I thought it proper to suppose, that a ring might be in such a situation as to render it almost invisible; and that, consequently, observations should not be given up, till a sufficient time had elapsed to obtain a better view of such a supposed ring, by a removal of the planet from its node. This has now sufficiently been obtained in the course of ten years; for, let the node of the ring have been in any situation whatsoever, provided it kept to the same, we must by this time have had a pretty good view of the ring itself. Placing therefore great confidence on the observation of March 5, 1792, supported by my late views of the planet, I venture to affirm, that it has no ring in the least resembling that, or rather those, of Saturn.

The flattening of the poles of the planet seems to be sufficiently ascertained by many observations. The 7-feet, the

10-feet, and the 20-feet instruments, equally confirm it; and the direction pointed out Feb. 26, 1794, seems to be conformable to the analogies that may be drawn from the situation of the equator of Saturn, and of Jupiter.

This being admitted, we may without hesitation conclude, that the Georgian planet also has a rotation upon its axis, of a considerable degree of velocity.

Reports and Observations relating to the Light and Size of the Georgian Satellites, and to their vanishing at certain Distances from the Planet.

Jan. 14, 1787. A star was put down, as a supposed very faint satellite; but, on the 17th, the planet being removed, it appeared nearly as bright as two considerable stars that had also been noted.

" Jan. 17. The 1st satellite is the smallest in appearance."

" Jan. 24. The 2d satellite is brighter than the first."

" Feb. 9, 1787. The 1st satellite is larger than the second."

Feb. 10. The planet was supposed to go to a triangle of pretty bright stars. The 11th it was between them, and the stars of the triangle were so dim, that, had they not been seen before, they might have been supposed to be satellites.

" Sept. 19, 1787. $4^h\ 24'$. I can still see the satellites, though " daylight is already very strong: they are fainter than the " faintest of Saturn's satellites." *

" Feb. 22, 1791. I cannot perceive the 1st satellite, probably " owing to its nearness to the planet."

" March 2, 1791. The 1st satellite is hardly to be seen; I

* Five satellites of Saturn were only known at that time.

"have however had several perfect glimpses of it. It seems to "be about the most contracted part of its orbit."

March 6. The supposed 3d and 4th satellites of March 5th were imagined to have been gone from their former places; but were seen the 7th, with this memorandum. "I mistook them last night for other stars, they being so large that I did not know them again."

"March 9. The 2d satellite is nearer the planet than the "first, and on that account appears smaller."

"Dec. 9, 1791. I do not perceive the 1st satellite."

"Feb. 13, 1792. $6^h\ 16'$. The 3d supposed satellite of last "night is a considerable star; not much less than *b*."

When the supposed third was pointed out the night before, it is said to be smaller than the 1st and 2d satellites. By the figure, it did not exceed the distance of the 2d; and *b* is called a pretty large star.

Feb. 20, 1792. The 2d satellite, being at a great distance, was mistaken for a pretty large star, till about four hours after, when its motion along with the planet was perceived.

"Feb. 21, 1792. $7^h\ 36'$. I cannot see the 2d satellite. By "calculation, it should be about $8^\circ,6$ S.F. and I suspect it to "be there, but cannot get the least assurance."

"March 15, 1792. I cannot see the 1st satellite with 300; nor with 480; nor with 600."

"March 19. $8^h\ 35'$. I cannot see the 2d satellite with 300. "With 480 I see it very well. I see it also with 800; and very "well with 1200. With 2400 and 4800 the satellite cannot be "seen; but there seems to be a whitish haziness coming on."

March 4, 1794. The 1st satellite could not be seen.

March 7. The 1st was invisible.

March 17. Both 1st and 2d were invisible.

March 21. The 1st was invisible, though looked for with all the powers of the instrument.

March 22. The 2d was hardly visible.

March 23. The 2d was not to be seen.

March 26. The 1st was but just visible.

March 5, 1796. The 2d was invisible.

April 4, 1796. The 1st was invisible.

" March 17, 1797. Power 600. Neither of the satellites are " visible to-night; with 300 I cannot see them. The night is " very beautiful, and I have a field bar to hide the planet; but, " notwithstanding this, I cannot see either of the satellites."

March 21. The 1st satellite was invisible.

March 23. The 2d was invisible. The 1st could not be seen immediately, but, having been informed where exactly to look for it, according to my calculation of its place, it was perceived; and with 600 seen very well.

March 25. Both satellites were invisible.

Remarks on the foregoing Observations.

From the observations of Jan. 14, Feb. 10, March 6, 1787, and Feb. 13, 1792, it appears, that all very small stars, when they come near the planet, lose much of their lustre. Indeed, every observation that has been recorded before, of supposed satellites that have been proved to be stars afterwards, has fully confirmed this circumstance; for they were always found to be considerable stars, and their being mistaken for satellites was owing to their loss of light when near the planet. This would hardly deserve notice, as it is well known that a superior light

will obstruct an inferior one; but some circumstances which attend the operation of the affections of light upon the eye, when objects are very faint, are so remarkable, that they must not be passed over in silence.

After having been used to follow up the satellites of Saturn and Jupiter, to the very margin of their planets, so as even to measure the apparent diameter of one of Jupiter's satellites by its entrance on the disk,* I was in hopes that a similar opportunity would soon have offered with the Georgian satellites: not indeed to measure the satellites, but to measure the planet itself, by means of the passage of the satellite over its disk. I expected also to have settled the epochs of the satellites, from their conjunctions and oppositions, with more accuracy than I have yet been able to do, from their various positions in other parts of their orbits. A disappointment of obtaining these capital advantages deserves to have its cause investigated; but, first of all, let us cast a look upon the observations.

The satellites, we may remark, become regularly invisible, when, after their elongation, they arrive to certain distances from the planet. In order to find what these distances are, we will take the first observation of this kind, as an example.

Feb. 22, 1791, the first satellite could not be seen. Now, by my lately constructed tables, its longitude from the apogee, at the time of observation, was 204,5 degrees; that is, 24,5 degrees from the most contracted part of its orbit, on the side that is turned to us, which, as its opposite is called the apogee, I shall call the perigee. By my tables also for the same day, we have the distance of the apogee from the planet, which is ,60; supposing the greatest elongation distance to be 1. This

* See Phil. Trans. for 1797, Part II. page 335.

being given, we may find an easy method of ascertaining the distance of the satellite, when it is near the apogee or perigee: for it will be sufficiently true for our purpose to use the following analogy. Cosine of the distance of the satellite from the apogee or perigee is to the apogee distance from the planet, as the greatest elongation is to the distance of the satellite from the planet. When the ellipsis is very open, this theorem will only hold good in moderate distances from the apogee or perigee; but, when it is a good deal flattened, it will not be considerably out in more distant situations: and it will also be sufficiently accurate to take the natural cosine from the tables to two places of decimals only. When this is applied to our present instance, we have ,91 for the natural cosine of 24,5 degrees; and the distance of the satellite from the planet will come out $\frac{,6 \times 33''}{,91} = 21'',8$.

By this method, it appears that the satellite, when it could not be seen, was nearly 22″ from the planet.

We must not however conclude, that this is the given distance at which it will always vanish. For instance, the same satellite, though hardly to be seen, was however not quite invisible March 2, 1791. Its distance from the planet, computed as before, was then only $\frac{,6 \times 33''}{1} = 19'',8$.

The clearness of the atmosphere, and other favourable circumstances, must certainly have great influence in observations of very faint objects; therefore, a computation of all the observations where the satellites were not seen, as well as a few others where they were seen, when pretty near the apogee or perigee, will be the surest way of settling the fact. The result of these computations is thus.

L 2

First satellite invisible.			Second satellite invisible.		
1791.	Feb. 22	at 21″,8	1792.	Feb. 21	at 23″,3
	Dec. 19	at 16,9	1794.	March 17	at 20,7
1792.	March 15	at 18,4		March 23	at 17,9
1794.	March 4	at 18,5	1796.	March 5	at 9,3
	March 7	at 12,5		March 17	at 6,3
	March 17	at 17,0		March 23	at 6,2
	March 21	at 15,5		March 25	at 8,7
	April 4	at 8,5			
1797.	March 17	at 4,8			
	March 21	at 4,6			
	March 25	at 4,8			
First satellite visible.			**Second satellite visible.**		
1791.	March 2	at 19″,8	1794.	March 22	at 17″,5
1794.	Feb. 26	at 14,1			

Thus, having the observations and calculated distances under our inspection, we find that both the satellites became always invisible when they were near the planet: that the 1st was generally lost when it came within 18″ of the planet, and the 2d at the distance of about 20″. In very uncommon and beautiful nights, the 1st has once been seen at 13″,8, and the 2d at 17″,3; but at no time have they been visible when nearer the planet.

I shall now endeavour to investigate the cause which can

render small stars and satellites invisible at so great a distance as 18 or 20″.

A dense atmosphere of the planet would account for the defalcation of light sufficiently, were it not proved that the satellites are equally lost, whether they are in the nearest half of their orbits, or in that which is farthest from us. But, as a satellite cannot be eclipsed by an atmosphere that is behind it, a surmise of this kind cannot be entertained. Let us then turn our view to light itself, and see whether certain affections between bright and very bright objects, contrasted with others that take place between faint and very faint ones, will not explain the phænomena of vanishing satellites.

The light of Jupiter or Saturn, for instance, on account of its brilliancy, is diffused, almost equally, over a space of several minutes all around these planets. Their satellites also, having a great share of brightness, and moving in a sphere that is strongly illuminated, cannot be much affected by their various distances from the planets. The case then is, that they have much light to lose, and comparatively lose but little.

The Georgian planet, on the contrary, is very faint; and the influence of its feeble light cannot extend far, with any degree of equality. This enables us to see the faintest objects, even when they are only a minute or two removed from it. The satellites of this planet are very nearly the dimmest objects that can be seen in the heavens; so that they cannot bear any considerable diminution of their light, by a contrast with a more luminous object, without becoming invisible. If then the sphere of illumination of our new planet be limited to 18 or 20″, we may fully account for the loss of the satellites when they come

within its reach; for they have very little light to lose, and lose it pretty suddenly.

This contrast, therefore, between the condition of the Georgian satellites and those of the brighter planets, seems to be sufficient to account for the phænomenon of their becoming invisible.

We may avail ourselves of the observations that relate to the distances at which the satellites vanish, to determine their relative brightness. The 2d satellite appears generally brighter than the 1st; but, as the former is usually lost farther from the planet than the latter, we may admit the 1st satellite to be rather brighter than the 2d. This seems to be confirmed by the observation of March 9, 1791; where the 2d appeared to be smaller than the 1st, though the latter was only 25″ from the planet, while the other was 30″,8.

The first of the new satellites will hardly ever be seen otherwise than about its greatest elongations, but cannot be much inferior in brightness to the other two; and, if any more interior satellites should exist, we shall probably not obtain a sight of them; for the same reason that the inhabitants of the Georgian planet perhaps never can discover the existence of our earth, Venus, and Mercury.

The 2d new or intermediate satellite is considerably smaller than the 1st and 2d old satellites. The two exterior, or 5th and 6th satellites, are the smallest of all, and must chiefly be looked for in their greatest elongations.

Periodical Revolutions of the new Satellites.

It may be some satisfaction to know what time the four

Fig. 1.

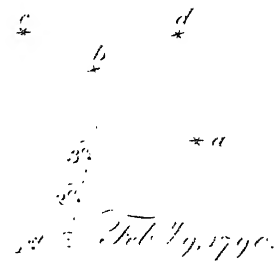


Fig. 2.

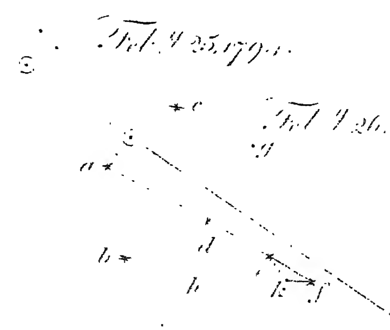


Fig. 3.

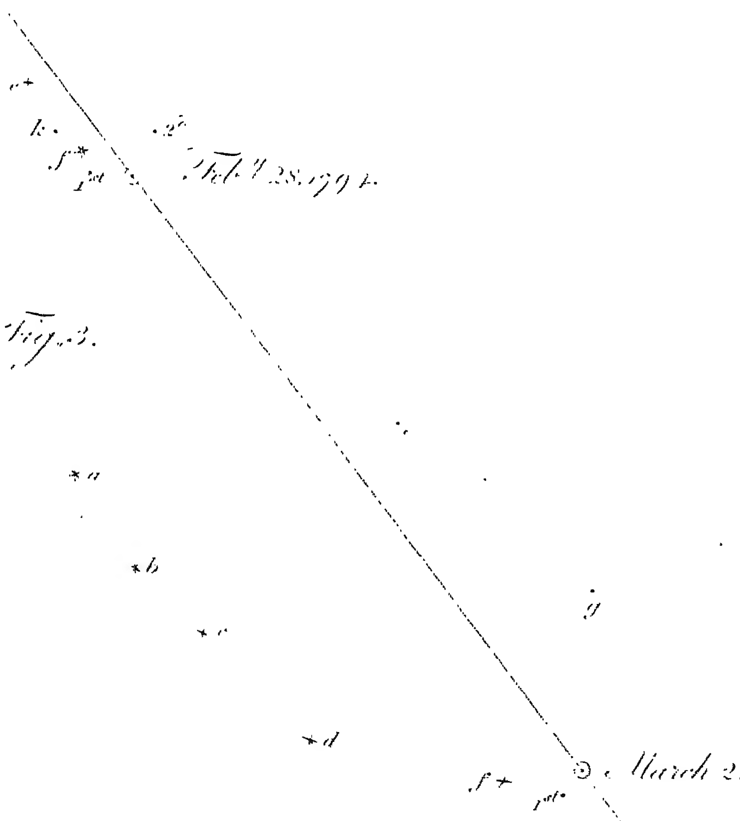
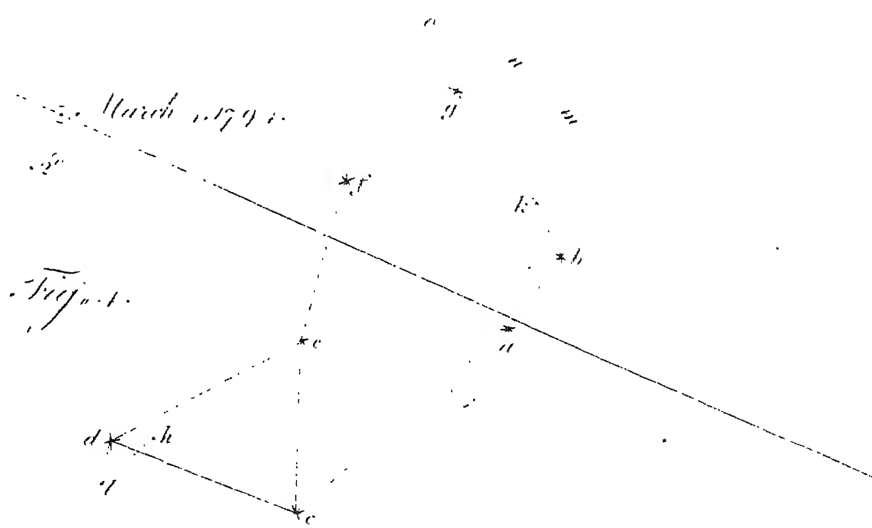


Fig. 4.



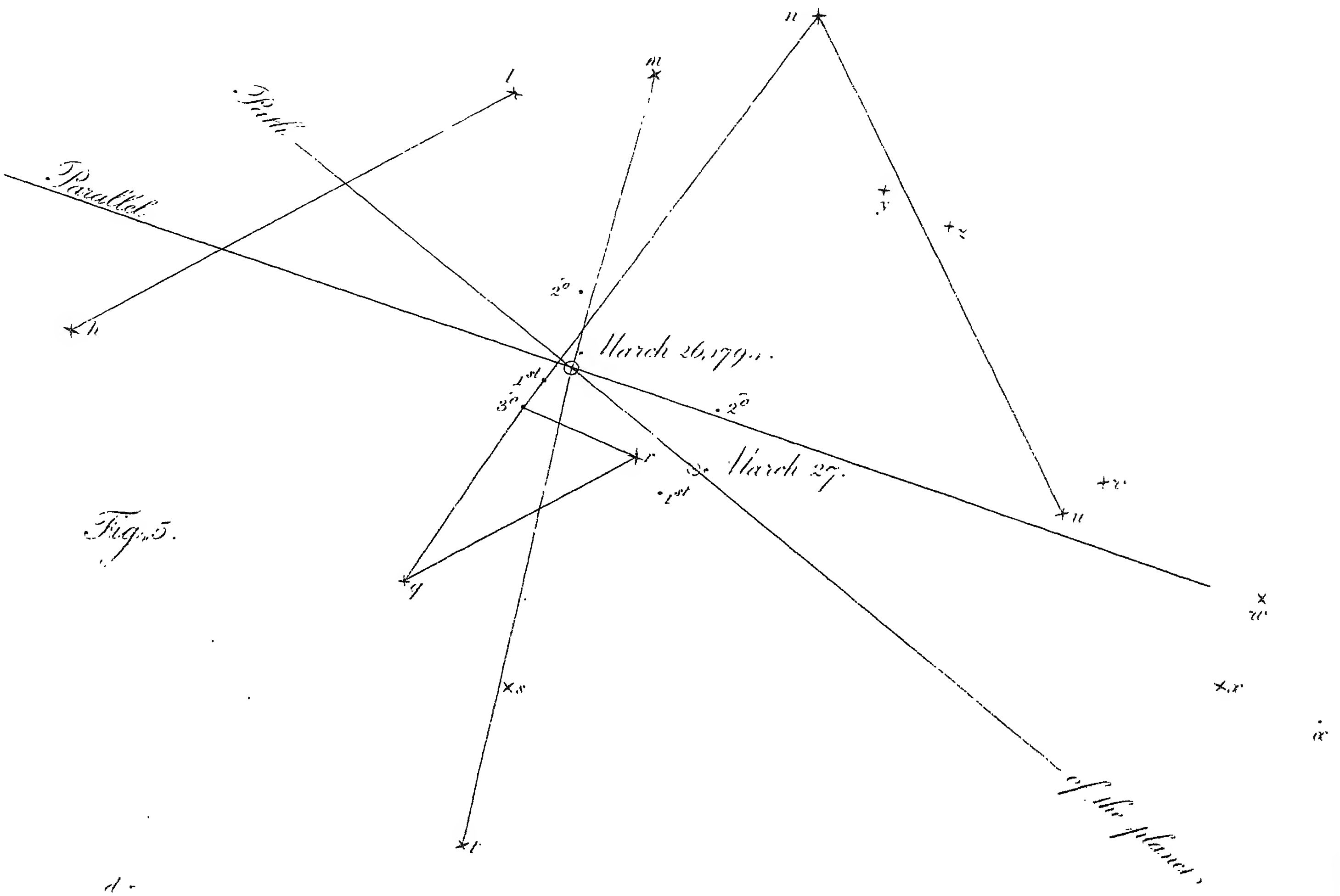


Fig. 5.

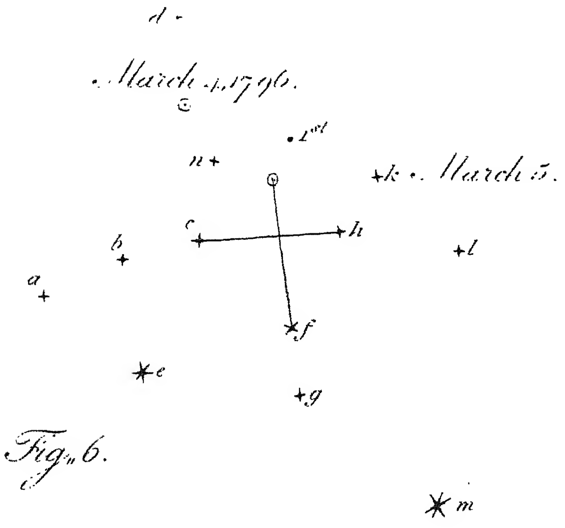


Fig. 6.


Fig. 7.


Fig. 8.


Fig. 9.


Fig. 10.

additional satellites probably employ in revolving round their planet. Now, as this can only be ascertained with accuracy by many observations, we must of course remain in suspense, till a series of them can be properly instituted. But, in the mean time, we may admit the distance of the interior satellite to be 25″,5, as our calculation of the estimation of March 5, 1794, gives it; and from this we compute that its periodical revolution will be 5 days, 21 hours, 25 minutes.

If we place the intermediate satellite at an equal distance between the two old ones, or at 38″,57, its period will be 10 days, 23 hours, 4 minutes.

By the figure of Feb. 9, 1790, it seems that the nearest exterior satellite is about double the distance of the farthest old one; hence, its periodical time is found to be 38 days, 1 hour, 49 minutes.

The most distant satellite, according to the calculation of the observation of Feb. 28, 1794, is full four times as far from the planet as the old 2d satellite; it will therefore take at least 107 days, 16 hours, 40 minutes, to complete one revolution.

It will hardly be necessary to add, that the accuracy of these periods depends entirely upon the truth of the assumed distances; some considerable difference, therefore, may be expected, when observations shall furnish us with proper *data* for more accurate determinations.

Slough, near Windsor,
September 1, 1797.

IV. *An Inquiry concerning the Source of the Heat which is excited by Friction.* *By* Benjamin *Count of* Rumford, *F. R. S. M. R. I. A.*

Read January 25, 1798.

It frequently happens, that in the ordinary affairs and occupations of life, opportunities present themselves of contemplating some of the most curious operations of nature; and very interesting philosophical experiments might often be made, almost without trouble or expence, by means of machinery contrived for the mere mechanical purposes of the arts and manufactures.

I have frequently had occasion to make this observation; and am persuaded, that a habit of keeping the eyes open to every thing that is going on in the ordinary course of the business of life has oftener led, as it were by accident, or in the playful excursions of the imagination, put into action by contemplating the most common appearances, to useful doubts, and sensible schemes for investigation and improvement, than all the more intense meditations of philosophers, in the hours expressly set apart for study.

It was by accident that I was led to make the experiments of which I am about to give an account; and, though they are not perhaps of sufficient importance to merit so formal an introduction, I cannot help flattering myself that they will be thought curious in several respects, and worthy of the honour of being made known to the Royal Society.

Being engaged, lately, in superintending the boring of cannon, in the workshops of the military arsenal at Munich, I was struck with the very considerable degree of heat which a brass gun acquires, in a short time, in being bored; and with the still more intense heat (much greater than that of boiling water, as I found by experiment,) of the metallic chips separated from it by the borer.

The more I meditated on these phænomena, the more they appeared to me to be curious and interesting. A thorough investigation of them seemed even to bid fair to give a farther insight into the hidden nature of heat; and to enable us to form some reasonable conjectures respecting the existence, or non-existence, of an *igneous fluid:* a subject on which the opinions of philosophers have, in all ages, been much divided.

In order that the Society may have clear and distinct ideas of the speculations and reasonings to which these appearances gave rise in my mind, and also of the specific objects of philosophical investigation they suggested to me, I must beg leave to state them at some length, and in such manner as I shall think best suited to answer this purpose.

From *whence comes* the heat actually produced in the mechanical operation above mentioned?

Is it furnished by the metallic chips which are separated by the borer from the solid mass of metal?

If this were the case, then, according to the modern doctrines of latent heat, and of caloric, the *capacity for heat* of the parts of the metal, so reduced to chips, ought not only to be changed, but the change undergone by them should be sufficiently great to account for *all* the heat produced.

But no such change had taken place; for I found, upon

taking equal quantities, by weight, of these chips, and of thin slips of the same block of metal separated by means of a fine saw, and putting them, at the same temperature, (that of boiling water,) into equal quantities of cold water, (that is to say, at the temperature of $59°\frac{1}{2}$ F.) the portion of water into which the chips were put was not, to all appearance, heated either less or more than the other portion, in which the slips of metal were put.

This experiment being repeated several times, the results were always so nearly the same, that I could not determine whether any, or what change, had been produced in the metal, *in regard to its capacity for heat*, by being reduced to chips by the borer.*

From hence it is evident, that the heat produced could not

* As these experiments are important, it may perhaps be agreeable to the Society to be made acquainted with them in their details.

One of them was as follows:

To 4590 grains of water, at the temperature of $59°\frac{1}{2}$ F. (an allowance as compensation, reckoned in water, for the capacity for heat of the containing cylindrical tin vessel, being included,) were added $1016\frac{1}{8}$ grains of gun-metal in thin slips, separated from the gun by means of a fine saw, being at the temperature of 210° F. When they had remained together 1 minute, and had been well stirred about, by means of a small rod of light wood, the heat of the mixture was found to be = 63°.

From this experiment, the *specific heat* of the metal, calculated according to the rule given by Dr. CRAWFORD, turns out to be = 0.1100, that of water being = 1.0000.

An experiment was afterwards made with the metallic chips, as follows:

To the same quantity of water as was used in the experiment above mentioned, at the same temperature, (*viz.* $59°\frac{1}{2}$,) and in the same cylindrical tin vessel, were now put $1016\frac{1}{8}$ grains of metallic chips of gun-metal, bored out of the same gun from which the slips used in the foregoing experiment were taken, and at the same temperature (210°). The heat of the mixture, at the end of 1 minute, was just 63°, as before; consequently the specific heat of these metallic chips was = 0.1100. Each of the above experiments was repeated 3 times, and always with nearly the same results.

possibly have been furnished at the expence of the latent heat of the metallic chips. But, not being willing to rest satisfied with these trials, however conclusive they appeared to me to be, I had recourse to the following still more decisive experiment.

Taking a cannon, (a brass six-pounder,) cast solid, and rough as it came from the foundry, (see fig. 1. Tab. IV.) and fixing it (horizontally) in the machine used for boring, and at the same time finishing the outside of the cannon by turning, (see fig. 2.) I caused its extremity to be cut off: and, by turning down the metal in that part, a solid cylinder was formed, $7\frac{3}{4}$ inches in diameter, and $9\frac{8}{10}$ inches long: which, when finished, remained joined to the rest of the metal (that which, properly speaking, constituted the cannon,) by a small cylindrical neck, only $2\frac{1}{5}$ inches in diameter, and $3\frac{8}{10}$ inches long.

This short cylinder, which was supported in its horizontal position, and turned round its axis, by means of the neck by which it remained united to the cannon, was now bored with the horizontal borer used in boring cannon; but its bore, which was 3.7 inches in diameter, instead of being continued through its whole length (9.8 inches) was only 7.2 inches in length; so that a solid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickness.

This cavity is represented by dotted lines in fig. 2; as also in fig. 3. where the cylinder is represented on an enlarged scale.

This cylinder being designed for the express purpose of generating heat *by friction*, by having a blunt borer forced against its solid bottom at the same time that it should be turned round its axis by the force of horses, in order that the heat accumu-

M 2

lated in the cylinder might from time to time be measured, a small round hole, (see *d, e,* fig. 3.) 0.37 of an inch only in diameter, and 4.2 inches in depth, for the purpose of introducing a small cylindrical mercurial thermometer, was made in it, on one side, in a direction perpendicular to the axis of the cylinder, and ending in the middle of the solid part of the metal which formed the bottom of its bore.

The solid contents of this hollow cylinder, exclusive of the cylindrical neck by which it remained united to the cannon, were $385\frac{3}{4}$ cubic inches, English measure; and it weighed 113.13lb. avoirdupois: as I found, on weighing it at the end of the course of experiments made with it, and after it had been separated from the cannon with which, during the experiments, it remained connected.*

Experiment No. 1.

This experiment was made in order to ascertain how much heat was actually generated by friction, when, a blunt steel borer being so forcibly shoved (by means of a strong screw) against the bottom of the bore of the cylinder, that the pressure

* For fear I should be suspected of prodigality in the prosecution of my philosophical researches, I think it necessary to inform the Society, that the cannon I made use of in this experiment was not sacrificed to it. The short hollow cylinder which was formed at the end of it, was turned out of a cylindrical mass of metal, about 2 feet in length, projecting beyond the muzzle of the gun, called in the German language the *verlorner kopf,* (the head of the cannon to be thrown away,) and which is represented in fig. 1.

This additional projection, which is cut off before the gun is bored, is always cast with it, in order that, by means of the pressure of its weight on the metal in the lower part of the mould, during the time it is cooling, the gun may be the more compact in the neighbourhood of the muzzle; where, without this precaution, the metal would be apt to be porous, or full of honeycombs.

against it was equal to the weight of about 10000lb. avoirdupois, the cylinder was turned round on its axis, (by the force of horses,) at the rate of about 32 times in a minute.

This machinery, as it was put together for the experiment, is represented by fig. 2. W is a strong horizontal iron bar, connected with proper machinery carried round by horses, by means of which the cannon was made to turn round its axis.

To prevent, as far as possible, the loss of any part of the heat that was generated in the experiment, the cylinder was well covered up with a fit coating of thick and warm flannel, which was carefully wrapped round it, and defended it on every side from the cold air of the atmosphere. This covering is not represented in the drawing of the apparatus, fig. 2.

I ought to mention, that the borer was a flat piece of hardened steel, 0.63 of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely, $3\frac{1}{2}$ inches. Its corners were rounded off at its end, so as to make it fit the hollow bottom of the bore; and it was firmly fastened to the iron bar (m) which kept it in its place. The area of the surface by which its end was in contact with the bottom of the bore of the cylinder was nearly $2\frac{1}{3}$ inches. This borer, which is distinguished by the letter n, is represented in most of the figures.

At the beginning of the experiment, the temperature of the air in the shade, as also that of the cylinder, was just 60° F.

At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horses being stopped, a cylindrical mercurial thermometer, whose bulb was $\frac{32}{100}$ of an inch in diameter, and $3\frac{1}{4}$ inches in length, was introduced into the

hole made to receive it, in the side of the cylinder, when the mercury rose almost instantly to 130°.

Though the heat could not be supposed to be quite equally distributed in every part of the cylinder, yet, as the length of the bulb of the thermometer was such that it extended from the axis of the cylinder to near its surface, the heat indicated by it could not be very different from that of the *mean temperature* of the cylinder; and it was on this account that a thermometer of that particular form was chosen for this experiment.

To see how fast the heat escaped out of the cylinder, (in order to be able to make a probable conjecture respecting the quantity given off by it, during the time the heat generated by the friction was accumulating,) the machinery standing still, I suffered the thermometer to remain in its place near three quarters of an hour, observing and noting down, at small intervals of time, the height of the temperature indicated by it.

Thus, at the end of		The heat, as shown by the thermometer, was
	4 minutes - - -	126°
after	5 minutes, always reckoning from the first observation, - -	125°
at the end of	7 minutes - - -	123°
	12 —— - - -	120°
	14 —— - - -	119°
	16 —— - - -	118°
	20 —— - - -	116°
	24 —— - - -	115°
	28 —— - - -	114°
	31 —— - - -	113°
	34 —— - - -	112°
	37½ —— - - -	111°
and when	41 minutes had elapsed - -	110°

Having taken away the borer, I now removed the metallic dust, or rather scaly matter, which had been detached from the bottom of the cylinder by the blunt steel borer, in this experiment; and, having carefully weighed it, I found its weight to be 837 grains Troy.

Is it possible that the very considerable quantity of heat that was produced in this experiment (a quantity which actually raised the temperature of above 113lb. of gun-metal at least 70 degrees of FAHRENHEIT's thermometer, and which, of course, would have been capable of melting $6\frac{1}{2}$lb. of ice, or of causing near 5lb. of ice-cold water to boil,) could have been furnished by so inconsiderable a quantity of metallic dust? and this merely in consequence of *a change* of its capacity for heat?

As the weight of this dust (837 grains Troy) amounted to no more than $\frac{1}{948}$th part of that of the cylinder, it must have lost no less than 948 degrees of heat, to have been able to have raised the temperature of the cylinder 1 degree; and consequently it must have given off 66360 degrees of heat, to have produced the effects which were actually found to have been produced in the experiment!

But, without insisting on the improbability of this supposition, we have only to recollect, that from the results of actual and decisive experiments, made for the express purpose of ascertaining that fact, the capacity for heat, of the metal of which great guns are cast, *is not sensibly changed* by being reduced to the form of metallic chips, in the operation of boring cannon; and there does not seem to be any reason to think that it can be much changed, if it be changed at all, in being reduced to much smaller pieces, by means of a borer that is less sharp.

If the heat, or any considerable part of it, were produced in consequence of a change in the capacity for heat of a part of the metal of the cylinder, as such change could only be *superficial*, the cylinder would by degrees be *exhausted;* or the quantities of heat produced, in any given short space of time, would be found to diminish gradually, in successive experiments. To find out if this really happened or not, I repeated the last-mentioned experiment several times, with the utmost care; but I did not discover the smallest sign of exhaustion in the metal, notwithstanding the large quantities of heat actually given off.

Finding so much reason to conclude, that the heat generated in these experiments, or *excited,* as I would rather choose to express it, was not furnished *at the expence of the latent heat* or *combined caloric* of the metal, I pushed my inquiries a step farther, and endeavoured to find out whether the air did, or did not, contribute any thing in the generation of it.

Experiment No. 2.

As the bore of the cylinder was cylindrical, and as the iron bar, (*m*,) to the end of which the blunt steel borer was fixed, was square, the air had free access to the inside of the bore, and even to the bottom of it, where the friction took place by which the heat was excited.

As neither the metallic chips produced in the ordinary course of the operation of boring brass cannon, nor the finer scaly particles produced in the last mentioned experiments by the friction of the blunt borer, showed any signs of calcination, I did not see how the air could possibly have been the cause

of the heat that was produced; but, in an investigation of this kind, I thought that no pains should be spared to clear away the rubbish, and leave the subject as naked and open to inspection as possible.

In order, by one decisive experiment, to determine whether the air of the atmosphere had any part, or not, in the generation of the heat, I contrived to repeat the experiment, under circumstances in which *it was evidently impossible for it to produce any effect whatever.* By means of a piston exactly fitted to the mouth of the bore of the cylinder, through the middle of which piston the square iron bar, to the end of which the blunt steel borer was fixed, passed in a square hole made perfectly air-tight, the access of the external air, to the inside of the bore of the cylinder, was effectually prevented. (In fig. 3. this piston (*p*) is seen in its place; it is likewise shown in fig. 7 and 8.)

I did not find, however, by this experiment, that the exclusion of the air diminished, in the smallest degree, the quantity of heat excited by the friction.

There still remained one doubt, which, though it appeared to me to be so slight as hardly to deserve any attention, I was however desirous to remove. The piston which closed the mouth of the bore of the cylinder, in order that it might be air-tight, was fitted into it with so much nicety, by means of its collars of leather, and pressed against it with so much force, that, notwithstanding its being oiled, it occasioned a considerable degree of friction, when the hollow cylinder was turned round its axis. Was not the heat produced, or at least some part of it, occasioned by this friction of the piston? and, as the external air had free access to the extremity of the bore, where it came in contact with the piston, is it not possible that

this air may have had some share in the generation of the heat produced?

Experiment No. 3.

A quadrangular oblong deal box, (see fig. 4.) water-tight, $11\frac{1}{2}$ English inches long, $9\frac{4}{10}$ inches wide, and $9\frac{6}{10}$ inches deep, (measured in the clear,) being provided, with holes or slits in the middle of each of its ends, just large enough to receive, the one, the square iron rod to the end of which the blunt steel borer was fastened, the other, the small cylindrical neck which joined the hollow cylinder to the cannon; when this box (which was occasionally closed above, by a wooden cover or lid moving on hinges,) was put into its place; that is to say, when, by means of the two vertical openings or slits in its two ends, (the upper parts of which openings were occasionally closed, by means of narrow pieces of wood sliding in vertical grooves,) the box (*g*, *h*, *i*, *k*, fig. 3.) was fixed to the machinery, in such a manner that its bottom (*i*, *k*,) being in the plane of the horizon, its axis coincided with the axis of the hollow metallic cylinder; it is evident, from the description, that the hollow metallic cylinder would occupy the middle of the box, without touching it on either side, (as it is represented in fig. 3.;) and that, on pouring water into the box, and filling it to the brim, the cylinder would be completely covered, and surrounded on every side, by that fluid. And farther, as the box was held fast by the strong square iron rod, (*m*,) which passed, in a *square hole*, in the centre of one of its ends, (*a*, fig. 4.) while the round or cylindrical neck, which joined the hollow cylinder to the end of the cannon, could turn round freely on its axis in the *round hole* in the centre of the other end of it, it is evident that the machinery could be put

in motion, without the least danger of forcing the box out of its place, throwing the water out of it, or deranging any part of the apparatus.

Every thing being ready, I proceeded to make the experiment I had projected, in the following manner.

The hollow cylinder having been previously cleaned out, and the inside of its bore wiped with a clean towel till it was quite dry, the square iron bar, with the blunt steel borer fixed to the end of it, was put into its place; the mouth of the bore of the cylinder being closed at the same time, by means of the circular piston, through the centre of which the iron bar passed.

This being done, the box was put in its place, and the joinings of the iron rod, and of the neck of the cylinder, with the two ends of the box, having been made water-tight, by means of collars of oiled leather, the box was filled with cold water, (*viz.* at the temperature of 60°.) and the machine was put in motion.

The result of this beautiful experiment was very striking, and the pleasure it afforded me amply repaid me for all the trouble I had had, in contriving and arranging the complicated machinery used in making it.

The cylinder, revolving at the rate of about 32 times in a minute, had been in motion but a short time, when I perceived, by putting my hand into the water, and touching the outside of the cylinder, that heat was generated; and it was not long before the water which surrounded the cylinder began to be sensibly warm.

At the end of 1 hour I found, by plunging a thermometer into the water in the box, (the quantity of which fluid amounted to 18.77lb. avoirdupois, or $2\frac{1}{4}$ wine gallons,) that its temperature

had been raised no less than 47 degrees; being now 107° of FAHRENHEIT'S scale.

When 30 minutes more had elapsed, or 1 hour and 30 minutes after the machinery had been put in motion, the heat of the water in the box was 142°.

At the end of 2 hours, reckoning from the beginning of the experiment, the temperature of the water was found to be raised to 178°.

At 2 hours 20 minutes it was at 200°; and at 2 hours 30 minutes it ACTUALLY BOILED!

It would be difficult to describe the surprise and astonishment expressed in the countenances of the by-standers, on seeing so large a quantity of cold water heated, and actually made to boil, without any fire.

Though there was, in fact, nothing that could justly be considered as surprising in this event, yet I acknowledge fairly that it afforded me a degree of childish pleasure, which, were I ambitious of the reputation of a *grave philosopher*, I ought most certainly rather to hide than to discover.

The quantity of heat excited and accumulated in this experiment was very considerable; for, not only the water in the box, but also the box itself, (which weighed $15\frac{1}{4}$lb.) and the hollow metallic cylinder, and that part of the iron bar which, being situated within the cavity of the box, was immersed in the water, were heated 150 degrees of FAHRENHEIT'S scale; *viz.* from 60° (which was the temperature of the water, and of the machinery, at the beginning of the experiment,) to 210°, the heat of boiling water at Munich.

The total quantity of heat generated may be estimated with some considerable degree of precision, as follows:

	Quantity of ice-cold water which, with the given quantity of heat, might have been heated 180 degrees, or made to boil. In avoirdupois weight.
Of the heat excited there appears to have been actually accumulated,	
In the water contained in the wooden box, $18\frac{3}{4}$ lb. avoirdupois, heated 150 degrees, namely, from 60° to 210° F. - - -	lb. 15.2
In 113.13 lb. of gun-metal, (the hollow cylinder,) heated 150 degrees; and, as the capacity for heat of this metal is to that of water as 0.1100 to 1.0000, this quantity of heat would have heated $12\frac{1}{2}$ lb. of water the same number of degrees - - -	10.37
In 36.75 cubic inches of iron, (being that part of the iron bar to which the borer was fixed which entered the box,) heated 150 degrees; which may be reckoned equal in capacity for heat to 1.21 lb. of water	1.01
N. B. No estimate is here made of the heat accumulated in the wooden box, nor of that dispersed during the experiment.	
Total quantity of ice-cold water which, with the heat actually generated by friction, and accumulated in 2 hours and 30 minutes, might have been heated 180 degrees, or made to boil - - -	26.58

From the knowledge of the *quantity* of heat actually produced in the foregoing experiment, and of the *time* in which it was generated, we are enabled to ascertain *the velocity of its production*, and to determine how large a fire must have been, or how much fuel must have been consumed, in order that, in burning equably, it should have produced by combustion the same quantity of heat in the same time.

In one of Dr. CRAWFORD's experiments, (see his Treatise on Heat, p. 321,) 37 lb. 7 oz. Troy, = 181920 grains, of water,

were heated $2\frac{1}{10}$ degrees of FAHRENHEIT's thermometer, with the heat generated in the combustion of 26 grains of wax. This gives 382032 grains of water heated 1 degree with 26 grains of wax; or $14693\frac{14}{26}$ grains of water heated 1 degree, or $\frac{14693}{180} = 81.631$ grains heated 180 degrees, with the heat generated in the combustion of 1 grain of wax.

The quantity of ice-cold water which might have been heated 180 degrees, with the heat generated by friction in the before-mentioned experiment, was found to be 26.58 lb. avoirdupois, = 188060 grains; and, as 81.631 grains of ice-cold water require the heat generated in the combustion of 1 grain of wax, to heat it 180 degrees, the former quantity of ice-cold water, namely 188060 grains, would require the combustion of no less than 2303.8 grains (= $4\frac{8}{10}$ oz. Troy) of wax, to heat it 180 degrees.

As the experiment (No. 3.) in which the given quantity of heat was generated by friction, lasted 2 hours and 30 minutes, = 150 minutes, it is necessary, for the purpose of ascertaining how many wax candles of any given size must burn together, in order that in the combustion of them the given quantity of heat may be generated in the given time, and consequently *with the same celerity* as that with which the heat was generated by friction in the experiment, that the size of the candles should be determined, and the quantity of wax consumed in a given time by each candle, in burning equably, should be known.

Now I found by an experiment, made on purpose to finish these computations, that when a good wax candle, of a moderate size, $\frac{3}{4}$ of an inch in diameter, burns with a clear flame, just 49 grains of wax are consumed in 30 minutes. Hence it appears, that 245 grains of wax would be consumed by such a candle in 150 minutes; and that, to burn the quantity of

wax (= 2303.8 grains) necessary to produce the quantity of heat actually obtained by friction in the experiment in question, and in the given time, (150 minutes,) *nine candles*, burning at once, would not be sufficient; for, 9 multiplied into 245 (the number of grains consumed by each candle in 150 minutes) amounts to no more than 2205 grains; whereas the quantity of wax necessary to be burnt, in order to produce the given quantity of heat, was found to be 2303.8 grains.

From the result of these computations it appears, that the quantity of heat produced equably, or in a continual stream, (if I may use that expression,) by the friction of the blunt steel borer against the bottom of the hollow metallic cylinder, in the experiment under consideration, was *greater* than that produced equably in the combustion of *nine wax candles*, each $\frac{3}{4}$ of an inch in diameter, all burning together, or at the same time, with clear bright flames.

As the machinery used in this experiment could easily be carried round by the force of one horse, (though, to render the work lighter, two horses were actually employed in doing it,) these computations show further how large a quantity of heat might be produced, by proper mechanical contrivance, merely by the strength of a horse, without either fire, light, combustion, or chemical decomposition; and, in a case of necessity, the heat thus produced might be used in cooking victuals.

But no circumstances can be imagined, in which this method of procuring heat would not be disadvantageous; for, more heat might be obtained by using the fodder necessary for the support of a horse, as fuel.

As soon as the last mentioned experiment (No. 3.) was finished, the water in the wooden box was let off, and the box

removed; and the borer being taken out of the cylinder, the scaly metallic powder, which had been produced by the friction of the borer against the bottom of the cylinder, was collected, and, being carefully weighed, was found to weigh 4145 grains, or about $8\frac{2}{3}$ oz. Troy.

As this quantity was produced in $2\frac{1}{2}$ hours, this gives 824 grains for the quantity produced *in half an hour.*

In the first experiment, which lasted only *half an hour*, the quantity produced was 837 grains.

In the experiment No. 1, the quantity of heat generated, in *half an hour*, was found to be equal to that which would be required to heat 5lb. avoirdupois of ice-cold water 180 degrees, or cause it to boil.

According to the result of the experiment No. 3, the heat generated in *half an hour*, would have caused 5.31 lb. of ice-cold water to boil. But, in this last-mentioned experiment, the heat generated being more effectually confined, less of it was lost; which accounts for the difference of the results of the two experiments.

It remains for me to give an account of one experiment more, which was made with this apparatus. I found by the experiment No. 1. how much heat was generated when the air had free access to the metallic surfaces which were rubbed together. By the experiment No. 2, I found that the quantity of heat generated was not sensibly diminished when the free access of the air was prevented; and, by the result of No. 3, it appeared that the generation of the heat was not prevented, or retarded, by keeping the apparatus immersed in water. But as, in this last-mentioned experiment, the water, though it surrounded the hollow metallic cylinder on every

side, externally, was not suffered to enter the cavity of its bore, (being prevented by the piston,) and consequently did not come into contact with the metallic surfaces where the heat was generated; to see what effects would be produced by giving the water free access to these surfaces, I now made the

Experiment No. 4.

The piston which closed the end of the bore of the cylinder being removed, the blunt borer and the cylinder were once more put together; and the box being fixed in its place, and filled with water, the machinery was again put in motion.

There was nothing in the result of this experiment that renders it necessary for me to be very particular in my account of it. Heat was generated, as in the former experiments, and, to all appearance, quite as rapidly; and I have no doubt but the water in the box would have been brought to boil, had the experiment been continued as long as the last. The only circumstance that surprised me was, to find how little difference was occasioned in the noise made by the borer in rubbing against the bottom of the bore of the cylinder, by filling the bore with water. This noise, which was very grating to the ear, and sometimes almost insupportable, was, as nearly as I could judge of it, quite as loud, and as disagreeable, when the surfaces rubbed together were wet with water, as when they were in contact with air.

By meditating on the results of all these experiments, we are naturally brought to that great question which has so often been the subject of speculation among philosophers; namely,

What is heat?—Is there any such thing as an *igneous fluid?*—Is there any thing that can with propriety be called *caloric?*

We have seen that a very considerable quantity of heat may be excited in the friction of two metallic surfaces, and given off in a constant stream or flux, *in all directions,* without interruption or intermission, and without any signs of diminution, or exhaustion.

From whence came the heat which was continually given off in this manner, in the foregoing experiments? Was it furnished by the small particles of metal, detached from the larger solid masses, on their being rubbed together? This, as we have already seen, could not possibly have been the case.

Was it furnished by the air? This could not have been the case; for, in three of the experiments, the machinery being kept immersed in water, the access of the air of the atmosphere was completely prevented.

Was it furnished by the water which surrounded the machinery? That this could not have been the case is evident; *first,* because this water was continually *receiving heat* from the machinery, and could not, at the same time, be *giving to,* and *receiving heat from,* the same body; and *secondly,* because there was no chemical decomposition of any part of this water. Had any such decomposition taken place, (which indeed could not reasonably have been expected,) one of its component elastic fluids (most probably inflammable air) must, at the same time, have been set at liberty, and, in making its escape into the atmosphere, would have been detected; but, though I frequently examined the water, to see if any air bubbles rose up through it, and had even made preparations for catching

them, in order to examine them, if any should appear, I could perceive none; nor was there any sign of decomposition of any kind whatever, or other chemical process, going on in the water.

Is it possible that the heat could have been supplied by means of the iron bar to the end of which the blunt steel borer was fixed? or by the small neck of gun-metal by which the hollow cylinder was united to the cannon? These suppositions appear more improbable even than either of those before mentioned; for heat was continually going off, or *out of the machinery*, by both these passages, during the whole time the experiment lasted.

And, in reasoning on this subject, we must not forget to consider that most remarkable circumstance, that the source of the heat generated by friction, in these experiments, appeared evidently to be *inexhaustible.*

It is hardly necessary to add, that any thing which any *insulated* body, or system of bodies, can continue to furnish *without limitation*, cannot possibly be a *material substance:* and it appears to me to be extremely difficult, if not quite impossible, to form any distinct idea of any thing, capable of being excited, and communicated, in the manner the heat was excited and communicated in these experiments, except it be MOTION.

I am very far from pretending to know how, or by what means, or mechanical contrivance, that particular kind of motion in bodies, which has been supposed to constitute heat, is excited, continued, and propagated, and I shall not presume to trouble the Society with mere conjectures; particularly on a subject which, during so many thousand years, the most

enlightened philosophers have endeavoured, but in vain, to comprehend.

But, although the mechanism of heat should, in fact, be one of those mysteries of nature which are beyond the reach of human intelligence, this ought by no means to discourage us, or even lessen our ardour, in our attempts to investigate the laws of its operations. How far can we advance in any of the paths which science has opened to us, before we find ourselves enveloped in those thick mists which, on every side, bound the horizon of the human intellect? But how ample, and how interesting, is the field that is given us to explore!

Nobody, surely, in his sober senses, has ever pretended to understand the mechanism of gravitation; and yet what sublime discoveries was our immortal NEWTON enabled to make, merely by the investigation of the laws of its action!

The effects produced in the world by the agency of heat, are probably *just as extensive*, and quite as important, as those which are owing to the tendency of the particles of matter towards each other; and there is no doubt but its operations are, in all cases, determined by laws equally immutable.

Before I finish this paper, I would beg leave to observe, that although, in treating the subject I have endeavoured to investigate, I have made no mention of the names of those who have gone over the same ground before me, nor of the success of their labours; this omission has not been owing to any want of respect for my predecessors, but was merely to avoid prolixity, and to be more at liberty to pursue, without interruption, the natural train of my own ideas.

DESCRIPTION OF THE FIGURES (Tab. IV.)

Fig. 1. shows the cannon used in the foregoing experiments, in the state it was in when it came from the foundry.

Fig. 2. shows the machinery used in the experiments No. 1, and No. 2. The cannon is seen fixed in the machine used for boring cannon. W is a strong iron bar, (which, to save room in the drawing, is represented as broken off,) which bar, being united with machinery (not expressed in the figure) that is carried round by horses, causes the cannon to turn round its axis.

m is a strong iron bar, to the end of which the blunt borer is fixed; which, by being forced against the bottom of the bore of the short hollow cylinder that remains connected by a small cylindrical neck to the end of the cannon, is used in generating heat by friction.

Fig. 3. shows, on an enlarged scale, the same hollow cylinder that is represented on a smaller scale in the foregoing figure. It is here seen connected with the wooden box (*g*, *h*, *i*, *k*,) used in the experiments No. 3, and No. 4, when this hollow cylinder was immersed in water.

p, which is marked by dotted lines, is the piston which closed the end of the bore of the cylinder.

n is the blunt borer seen side-wise.

d, *e*, is the small hole by which the thermometer was introduced, that was used for ascertaining the heat of the cylinder. To save room in the drawing, the cannon is represented broken off near its muzzle; and the iron bar, to which the blunt borer is fixed, is represented broken off at *m*.

Fig. 4. is a perspective view of the wooden box, a section of which is seen in the foregoing figure, (see *g*, *h*, *i*, *k*, fig. 3.)

Fig. 5 and 6. represent the blunt borer *n*, joined to the iron bar *m*, to which it was fastened.

Fig. 7 and 8. represent the same borer, with its iron bar, together with the piston which, in the experiments No. 2 and No. 3, was used to close the mouth of the hollow cylinder.

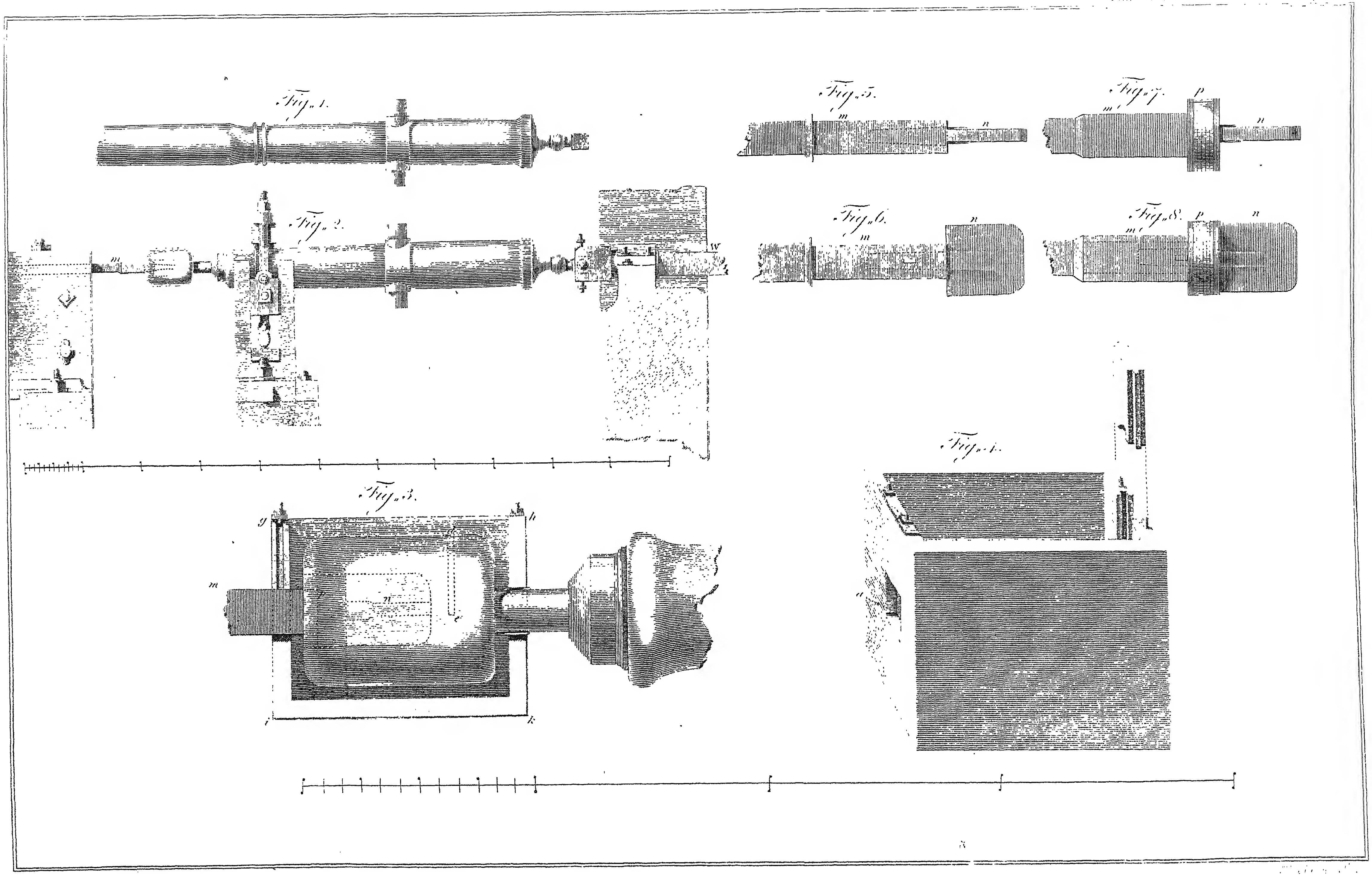


Fig. 1.
Fig. 2.
m
w
Fig. 3.
g
h
m
p
n
c
i
k
Fig. 4.
a
Fig. 5.
m
n
Fig. 6.
m
n
Fig. 7.
m
p
n
Fig. 8.
m
p
n

V. *Observations on the* Foramina Thebesii *of the Heart. By Mr.* John Abernethy, *F. R. S. Communicated by* Everard Home, *Esq. F. R. S.*

Read February 1, 1798.

As the investigation of the resources of nature in the animal œconomy, for the maintenance of health, and the prevention of disease, cannot but be interesting to the philosopher as well as to the physician, I therefore am induced to submit to the Society the following observations.

There is a remarkable contrivance in the blood vessels which supply the heart, not to be met with in any other part of the body, and which is of great use in the healthy functions of that organ, but which is particularly serviceable in preventing disease of a part so essential to life.

A distended state of the blood vessels must always impede their functions, and consequently be very detrimental to the health of the part which they supply; but, as the cavities of the heart are naturally receptacles of blood, a singular opportunity is afforded to its nutrient vessels, to relieve themselves when surcharged, by pouring a part of their contents into those cavities. Such appears to be the use of the foramina by which injections, thrown into the blood vessels of the heart, escape into the cavities of that organ; and which were first noticed by Vieussens, but, being more expressly described by Thebesius, generally bear the name of the latter author.

Anatomists appear to have been much perplexed concerning these *foramina Thebesii;* even HALLER, SENAC, and ZINN, were sometimes unable to discover them; which suggested an idea, that when an injection was effused into the cavities of the heart, the vessels were torn, and that it did not escape through natural openings. When these foramina were injected, they were found under various circumstances, as to their size and situation; and HALLER observed, that the injection, for the most part, escaped into the right cavities of the heart. It also remains undetermined, whether these foramina belong both to the arteries and veins, or respectively to each set of vessels.

It is from an examination of these openings in diseased subjects, that a solution of such difficulties may probably be obtained. Whoever reflects on the circumstances under which the principal coronary vein terminates in the right auricle of the heart, will perceive that an impediment to the flow of blood through that vessel must occasionally take place; but the difficulty will be much increased, when the right side of the heart is more than ordinarily distended, in consequence of obstruction to the pulmonary circulation. Indeed it seems probable, that such an obstruction, by occasioning a distended state of the right side of the heart, and thus impeding the circulation in the nutrient vessels of that organ, would as necessarily occasion corresponding disease in it, as an obstruction to the circulation in the liver occasions disease in the other abdominal viscera, were it not for some preventing circumstances, which I now proceed to explain.

Having been attentive to some very bad cases of pulmonary consumption, from a desire to witness the effects of breathing medicated air in that complaint, I was led to a more particular

examination of the heart of those patients who died. In these cases, I found, that by throwing common coarse waxen injection into the arteries and veins of the heart, it readily flowed into the cavities of that organ; and that the left ventricle was injected in the first place, and most completely. When the ventricle was opened, and the effused injection removed, the foramina Thebesii appeared both numerous and large, and distended with the different coloured wax which had been impelled into the coronary arteries and veins. Upon eight comparative trials, made by injecting the vessels of hearts taken from subjects whose lungs were either much diseased, or in a perfectly sound state, I found, that in the former, common injection readily flowed, in the manner which I have described, into all the cavities of the heart, but principally into the left ventricle; whilst, in many of the latter, I could not impel the least quantity of such coarse injection into that cavity.

This difference in the facility with which the cavities of the heart can be injected from its nutrient vessels, was observed by most anatomists, though they did not advert to the circumstances on which it depended. HALLER's recital of his own observations, and of those of others on this subject, so well explain the facts which I have stated, that I shall take the liberty of quoting the passage, in order further to illustrate and authenticate them. He says, "Si per arterias liquorem in-"jeceris, perinde in dextra auricula, sinuque et ventriculo dextro, "et in sinu atque thalamo sinistro guttulæ exstillabunt; sæpe "quidem absque mora, alias difficilius, et nonnunquam omnino, "uti continuo dicemus, et mihi, et SENACO, et clarissimo ZINNIO, "nihil exsudavit."—*Elem. Physiol. Tom.* I. *page* 382.

As it seems right that the blood which had been distributed

by the coronary arteries, and which must have lost, in a greater or less degree, the properties of arterial blood, should not be mixed with the arterial blood which is to be distributed to every part of the body, but ought rather to be sent again to the lungs, in order that it may re-acquire those properties; we therefore perceive why, in a natural state of the heart, the principal foramina Thebesii are to be found in the *right* cavities of that organ. However, as, even in a state of health, those cavities are liable to be uncommonly distended, in consequence of muscular exertion sometimes forcing the venous blood into the heart faster than it can be transmitted through the lungs, there seems to arise a necessity for similar openings on the left side; but these, in their natural state, though capable of emitting blood, and of relieving the plethora of the coronary vessels, are not of sufficient size to give passage to common waxen injections. Yet, when there is a distended state of the right cavities of the heart, which is almost certainly occasioned by a diseased state of the lungs, these foramina leading into the left cavities then become enlarged, in the manner that has been already described; and thus the plethoric state of the nutrient vessels of the heart, and the consequent disease of that important organ, are prevented.

The preceding remarks will, I think, sufficiently explain the cause of the variety in the size and situation of these foramina, which also appear to belong both to the arteries and veins; because, the injection which was employed was too coarse to pass from one set of vessels to the other, and yet the different coloured injections passed into the cavities of the heart unmixed.

There is yet another mode by which diseases of the heart, that would otherwise so inevitably succeed to obstruction in the

pulmonary vessels, are avoided; and which I next beg leave to explain.

Having formerly been much surprised to find the heart so little affected, when the lungs were greatly diseased, and observing, in one or two instances, that the *foramen ovale* was open, I was led to pay more particular attention to the state of that part; and I have found this to be almost a constant occurrence in those subjects where pulmonary consumption had for some time existed previous to the person's decease. I took notice of this circumstance thirteen times in the course of one year: and, in several instances, the aperture was sufficiently large to admit of a finger being passed through it. Now, as the *septum auricularum* is almost constantly perfect in subjects whose lungs are healthy, I cannot but conclude, that the renewal of the *foramen ovale* is the effect of disease: nor will the opinion appear, on reflection, improbable; for the opening becomes closed by the membranous fold growing from one edge of it, till it overlaps the other, and their smooth surfaces being kept in close contact, by the pressure of the blood in the left auricle, they gradually grow together. But, should there be a deficiency of blood in the left auricle, and a redundance in the right, the pressure of the latter on this membranous partition, will so stretch and irritate the uniting medium, as to occasion its removal; and thus a renewal of the communication between the auricles will again take place.

From these observations it is natural to suppose, that in those men, or animals, who are accustomed to remain long under water, this opening will either be maintained or renewed: yet on this circumstance alone the continuance of their life does not depend; for we now have sufficient proof, that if the

blood is not oxygenated in the lungs, it is unfit to support the animal powers. There is an experiment related by Buffon, the truth of which, I believe, has not been publicly controverted, and which tends greatly to misrepresent this subject. He says, that he caused a bitch to bring forth her puppies under warm water; that he suddenly removed them into a pail of warm milk; that he kept them immersed in the milk for more than half an hour; and that when they were taken out of it, all the three were alive. He then allowed them to respire about half an hour, and again immersed them in the warm milk, where they remained another half hour; and, when taken out, two were vigorous, but the third seemed to languish: this submersion was again repeated, without apparent injury to the animals.

This experiment is so directly contrary to what we are led to believe from all others, and also to the information derived from cases which frequently occur in the practice of midwifery, (in which, an interruption to the circulation through the umbilical chord occasions the death of the fœtus,) as to make me suspect its truth: I was therefore induced to examine what would happen in a similar experiment. I did not indeed cause the bitch to bring forth her puppies in water; but immersed a puppy, shortly after its birth, under water which was of the animal temperature. It lost all power of supporting itself in about 60 seconds, and would shortly have perished, had I not removed it into the air. Neither could I, by repeating this experiment, so accustom the animal to the circulation of unoxygenated blood, as to lengthen the term of its existence in such an unnatural situation. I thought that a dog might have been made a good diver in this way; but, having satisfied myself

that this could not be done, without greatly torturing the animal, I did not choose to prosecute so cruel an experiment.

Young animals, indeed, retain their irritability for a considerable time, so that they move long after they have been plunged beneath water; and may even, on this account, recover after they are taken out. But the manner in which BUFFON has related his experiment seems to imply, that the circulation of the blood, and other functions of life, were continued after the animals had been excluded from the air. I am convinced that the poor dog who was the subject of my experiment would have been beyond recovery in a few minutes.

Those animals who are accustomed to remain long under water, probably first fill their lungs with air, which may, in a partial manner, oxygenate their blood during their submersion. The true statement of this subject may probably be, that the circulation of venous blood will destroy most animals in a very short space of time; but that custom may enable others to endure it, with very little change, for a longer period.

VI. *An Analysis of the earthy Substance from New South Wales, called* Sydneia *or* Terra Australis. *By* Charles Hatchett, *Esq. F. R. S.*

Read February 8, 1798.

§. 1.

The late ingenious Josiah Wedgwood, Esq. F.R.S. published, in the Philosophical Transactions for the year 1790, an account of some analytical experiments on a mineral substance from Sydney Cove, in New South Wales.*

This substance, Mr. Wedgwood describes to be composed of a fine white sand, a soft white earth, some colourless micaceous particles, and also some which were black, resembling black mica, or black lead.

Nitric acid did not appear to act on any part of this earthy substance; and even a portion on which sulphuric acid had been boiled to dryness, afforded afterwards, when edulcorated with water, only a few flocculi, which Mr. Wedgwood conceived to be aluminous earth.

The muriatic acid, during digestion, seemed to act as little as the two preceding acids; but, upon water being poured in, to wash out the remaining portion, the liquor instantly became white as milk, with a fine white curdy substance intermixed; the concentrated acid having, in the opinion of the author,

* Philosophical Transactions, Vol. LXXX. Part II. page 306.

extracted something which the simple dilution with water precipitated.

The remaining part was repeatedly digested with muriatic acid, and treated with water, as before, till the milky appearance was no longer produced.

The properties of this white precipitate, Mr. WEDGWOOD states to be as follows.

1st. It is only soluble in boiling concentrated muriatic acid.

2dly. It is precipitated by water, in the form of a white earth; which may again be dissolved by boiling muriatic acid.

3dly. When nitric acid is mixed with the muriatic solution of this earth, there is no appearance of a precipitate; not even when water is added, provided that the nitric acid exceeds, or nearly approaches, the quantity of muriatic acid.

4thly. The earth is precipitated by the alkalies.

5thly. The muriatic solution does not crystallize by evaporation; but becomes a butyraceous mass, which soon liquefies on exposure to the air.

6thly. The butyraceous mass is not corrosive to the taste; and is even less pungent than the combination of calcareous earth with the same acid.

7thly. Heat approaching to ignition disengages the acid from the butyraceous mass, in white fumes, and a white substance remains.

8thly. The white precipitated earth is fusible *per se*, in from 142° to 156° of Mr. WEDGWOOD'S thermometer, and it is thus distinguished from all the other primitive earths.

And, 9thly. This precipitate cannot be reduced to a metallic state, when exposed to heat with inflammable substances.

From these properties, Mr. WEDGWOOD says, that although

he cannot absolutely determine whether this substance belongs to the class of earths, or that of metallic substances, yet he is inclined to refer it to the former.

Professor BLUMENBACH, of Göttingen, in his Manual of Natural History, published in 1791, also mentions that he had examined a portion of this earthy substance, by means of muriatic acid, after the manner of Mr. WEDGWOOD, and that he had obtained a slight precipitate by the addition of water.*

In consequence of these experiments, the mineralogists throughout Europe admitted the white precipitated substance to be a primitive earth; and we accordingly find, in all the systematical works on mineralogy published since the above-mentioned period, that it is arranged as a distinct genus, under the names of *Sydneia, Australa, Terra Australis,* and *Austral Sand.*

The extreme scarcity of this substance prevented the chemists in general from examining more minutely into the nature of this new primitive earth, till Mr. KLAPROTH, in the second volume of his Additions to the Chemical Knowledge of Mineral Bodies, gave to the public a memoir entitled, A Chemical Examination of the *Austral Sand.*†

In this memoir, Mr. KLAPROTH says, that he had received from Mr. HAIDINGER, of Vienna, two samples of this substance; one of which had a considerable quantity of black shining particles intermixed with it, which, although regarded by many as graphite or plumbago, he was inclined to believe to be *Eisenglimmer* or micaceous iron ore.

The other contained much less of these black or dark grey

* *Handbuch der Naturgeschichte,* p. 567 and 568.

† *Beiträge zur Chemischen Kenntniss der Mineralkörper.—Zweiter Band,* p. 66.

particles, and, as he considered it to be more pure than the former, he subjected it to the following experiments.

1. It was digested at three different times with concentrated muriatic acid, in a boiling heat, and the acid was afterwards filtrated through paper. The solution was then mixed by degrees with pure water, which did not however produce any precipitate, even when warmed.

Carbonate of potash caused some flocculi to fall, which, edulcorated and dried, weighed 3.25 grains.

This precipitate was dissolved in diluted sulphuric acid, and left a small portion of siliceous earth; after which the solution, by evaporation, afforded crystals of alum.

2. The residuum of the muriatic solution was mixed with three times the weight of potash, and exposed to a red heat. Muriatic acid was then poured on the mass, and the insoluble gelatinous residuum was edulcorated on a filter; and, after a red heat, weighed 19.50 grains, which proved to be siliceous earth.

3. The muriatic solution, with prussiate of potash, afforded a blue precipitate; the ferruginous part of which was about one quarter of a grain.

4. The solution was then saturated with carbonate of potash, and some alumine was precipitated; which, after a red heat, weighed 8.50 grains, and with sulphuric acid formed alum.

Siliceous earth, alumine, and iron, appeared therefore to be the only ingredients of this substance; but, as Mr. Klaproth had no more than thirty grains to examine, he could not extend his experiments.

From those above related, he is of opinion that the existence

of this primitive earth may be much doubted, and that this doubt can only be removed in the course of time, by other analyses.

Mr. KLAPROTH concludes his memoir by saying, that the substance examined by him was undoubtedly the genuine austral sand, as Mr. HAIDINGER had received it from Sir JOSEPH BANKS, when he was in London.

Mr. NICHOLSON, however, in the 9th Number of his Journal of Natural Philosophy, *&c.* (p. 410.) published on the 1st of December, 1797, questions much, whether the substance examined by Mr. KLAPROTH was the same as that examined by Mr. WEDGWOOD; and, after having contrasted their experiments, says, "hence it seems fair to conclude that the two "minerals were not the same, however this may have happened; and that the existence of the new fusible earth of "WEDGWOOD stands on the same evidence as before, namely, "his experiments, which have not yet been repeated, that I "know of."

Some of Mr. NICHOLSON'S objections to the experiments of Mr. KLAPROTH, being founded principally on some difference in the external characters of the substance examined by him, and the one examined by Mr. WEDGWOOD, are such as very naturally occur; but the following pages will, I believe, prove that Mr. KLAPROTH'S experiments were made on that which might be justly regarded as the *Sydneia* or austral sand.

In 1796, the Right Hon. Sir JOSEPH BANKS, P. R. S. favoured me with a specimen of the *Sydneia*, which had been lately brought to England; a portion of this I soon after examined, in a cursory manner, by muriatic acid, but did not obtain any precipitate when water was added to the filtrated solution.

Upon mentioning this circumstance, and expressing a desire to examine this substance with more accuracy, Sir JOSEPH BANKS, with his usual readiness to promote every scientific inquiry, not only permitted me to take specimens from different parts of the box which contained the earth already mentioned, but (that every doubt might be obviated) gave me about 300 grains which remained of the identical substance examined by Mr. WEDGWOOD.

Upon these the following experiments were made; and, to distinguish them, I shall call the first, No. 1, and that examined by Mr. WEDGWOOD, No. 2.

§. 2.

Analysis of the Sydneia, *No.* 1.

The *Sydneia*, No. 1, is in masses and lumps, of a pale greyish white, intermixed with a few particles of white mica, and also occasionally with some which are of a dark grey, resembling graphite or plumbago.

It easily crumbles between the fingers, to a powder nearly impalpable, which has rather an unctuous feel.

Small fragments of vegetable matter are also commonly found intermixed with it; and the general aspect is that of an earthy substance which has been deposited by water.

EXPERIMENT 1.

400 Grains were put into a glass matrass, and one quart of distilled water being added, the whole was boiled to one fourth.

The liquor was then filtrated, and a portion being examined by the re-agents commonly used, afforded no trace of matter

in solution. The remainder was then evaporated, without leaving any residuum.

EXPERIMENT 2.

About 200 grains of the earth, rubbed to a fine powder, were put into a glass retort, into which I poured three ounces of concentrated pure muriatic acid. The retort was placed in sand, and the acid was distilled, till the matter in the retort remained dry. Two ounces of muriatic acid were again poured on it, and distilled as before, till only one fourth remained. The whole was then put into a matrass, which was placed in an inclined position, so that when the earth had subsided, the liquor might be decanted, without disturbing the sediment.

When it had remained thus for 12 hours, the acid was carefully poured into a glass vessel; but, as I observed that it was not so perfectly transparent as before it had been thus employed, I suffered it to remain 24 hours, but did not perceive any sediment. Half of this liquor was diluted with about twelve parts of distilled water, and, after a few hours, a very small quantity of a white earth subsided.

This however did not appear to me to be a precipitate caused by a change in the chemical affinities, but rather an earthy matter which had been suspended in the concentrated acid, and afterwards deposited, when the liquor was rendered less dense by the addition of water. To ascertain this, I poured the remaining portion of the concentrated liquor on a filter of four folds: it passed perfectly transparent, and, although diluted with twenty-four parts of water, it remained unchanged, and as pellucid as before. I now filtrated the former portion, and added it to that already mentioned.

It was then evaporated to dryness, and left a pale brownish mass, which was dissolved again, by digestion, in the smallest possible quantity of muriatic acid.

Water was added, in a very large proportion, to this solution, without producing any effect; I then, by prussiate of potash, precipitated a quantity of iron, which was separated by a filter.

The clear solution was then saturated with lixivium of carbonate of potash, and a white precipitate was produced, which was collected and edulcorated. This, when digested with diluted sulphuric acid, was dissolved; and the superfluous acid being driven off by heat, boiling water was poured on the residuum, and completely dissolved it.

To this solution some drops of lixivium of potash were added, and, by repeated evaporations, the whole formed crystals of alum.

From the above experiment it appeared, that the muriatic acid had only dissolved some alumine and iron; but, in order to satisfy myself more completely in respect to the component parts of this substance, I made the following analysis.

Analysis. A. 400 grains were put into a glass retort, which was then made red-hot during half an hour. Some water came over, and the earth afterwards weighed 380.80 grains, so that the loss amounted to 19.20 grains. The greater part of this loss was occasioned by the dissipation of the water imbibed by the earth; to which must be added, the loss of weight caused by the combustion of a small portion of vegetable matter.

B. The 380.80 grains were rubbed to a fine powder, and being put into a glass retort, 1470 grains of pure concentrated sulphuric acid were added. The retort was then placed in a small reverberatory, and the fire was gradually increased, till

the acid was distilled over: it was then poured back on the matter in the retort, and distilled as before, till a mass nearly dry remained.

On this, boiling distilled water was repeatedly poured, until it no longer changed the colour of litmus paper, and was devoid of taste. The undissolved portion was then dried, and made red-hot; after which it weighed 281 grains.

C. I now mixed the 281 grains with 300 grains of dry carbonate of potash, and exposed the mixture to a strong red heat, in a silver crucible, during four hours. The mass was loose, and of a greyish white: it was softened with water, and, being put into a retort, sulphuric acid was added to a considerable excess. The whole was then distilled to dryness, and, when a sufficient quantity of boiling water had been added, it was poured on a filter, and the residuum was well washed; it was then made red-hot, and afterwards weighed 274.75 grains.

D. The solutions of B and C were added together, and were much reduced by evaporation. Pure ammoniac was then employed to saturate the acid, and a copious loose precipitate, of a pale yellowish colour was produced; which, collected, edulcorated, and made red-hot, weighed 103.70 grains.

E. The filtrated liquor of D was again evaporated, and carbonate of potash being added, a slight precipitation of earthy matter took place; which, by the test of sulphuric acid, proved to be some alumine which had not been precipitated in the former experiment: this weighed 1.20 grain.

F. The 103.70 grains of D were completely dissolved when digested with nitric acid, excepting a small residuum of siliceous earth, which weighed 0.90 grain.

G. The nitric solution was evaporated to dryness, and a

second portion of the same acid was added, and in like manner evaporated. The residuum was then made red-hot, and digested with diluted nitric acid, which left a considerable portion of red oxide of iron. The solution was again evaporated, and the residuum, being treated as before, again deposited some oxide of iron, much less in quantity than the former.

The whole of the oxide was then heated with wax in a porcelain crucible, was taken up by a magnet, and weighed 26.50 grains.

H. The nitric solution of G was saturated with ammoniac, and a loose white precipitate was formed; which, edulcorated and made red-hot, weighed 76 grains.

I. These 76 grains were dissolved when digested with diluted sulphuric acid; and, when the excess of acid had been expelled by heat, the saline mass was dissolved in boiling water. To this solution I added some lixivium of potash, and, by gradual and repeated evaporations, obtained the whole in regular octoedral crystals of alum.

K. The 274.75 grains of C now alone remained to be examined. They appeared to consist of siliceous earth, mixed with the dark grey shining particles already mentioned; but, as I shall describe, in the following experiments, the process by which these were separated, I shall now only say that they amounted to 7.50 grains.

L. The earth with which the abovementioned particles were mixed weighed 267.25 grains This earth was white, and arid to the touch: when melted with two parts of soda, it formed a colourless glass; and, with four parts of the same it dissolved in water, and formed a *liquor silicum:* it was therefore pure siliceous earth or silica.

The substance here examined was composed therefore of the following ingredients.

			grains.
Pure siliceous earth or silica -	{	F.	0.90
		L.	267.25
Alumine - - - -	{	E.	1.20
		H.	76
Oxide of iron - - -		G.	26.50
Dark grey particles - -		K.	7.50
Water and vegetable matter -		A.	19.20
			398.55

The foregoing analysis was repeated several times, and always with similar results; excepting, that as I had taken the specimens from different parts of a large quantity, I found that the proportions of the ingredients were not constantly the same: that of the siliceous earth, for example, was sometimes greater, and the alumine and iron proportionably less. Some specimens were also nearly or totally destitute of the dark grey shining particles; in short, every circumstance was such as might be expected from a mixed substance, which, from the nature of its formation, cannot have the ingredients in any fixed proportion.*

As this substance agreed in its general characters, for the greater part, with that described by Mr. WEDGWOOD, and as it was indisputably brought from the same place, there appeared every reason to believe that the nature of both was the same;

* The description given by Mr. KLAPROTH convinces me, that his experiments were made on a portion of this substance. Moreover, when my late friend Mr. HAIDINGER was in London, I gave him some of this earth for his collection; so that, whether Mr. KLAPROTH made his experiments on that which had been received by Mr. HAIDINGER from Sir JOSEPH BANKS, or from myself, it is not less certain that he operated on that which might be regarded as the genuine *Sydneia*.

but, to obviate as much as possible any doubt or objection, I determined to repeat the experiments, and the analysis, on that portion which remained of the identical substance examined by Mr. WEDGWOOD, and which from that period had been reserved by Sir JOSEPH BANKS, who kindly favoured me with it for this purpose.

§. 3.

Analysis of the Sydneia, *No.* 2.

This substance, as has already been mentioned, consists of a white transparent quartzose sand, a soft opaque white earth, some particles of white mica, and a quantity of dark lead-grey particles, which have a metallic lustre.

The Sydneia, No. 2, appears chiefly to differ from No. 1, by being more arenaceous, and by a larger proportion of the dark grey particles. Many experiments, similar to those made on No. 1, already described, were made on this substance, with pure concentrated muriatic acid; but, as none of these afforded any appearance of a precipitate by the means of water, I do not think it necessary to enter into a circumstantial account of them, and shall proceed therefore to the analysis.

A. 100 grains were exposed to a red heat, in a glass retort, and, after half an hour, were found to have lost in weight 2.20 grains.

B. The 97.80 grains which remained were mixed with 300 grains of dry carbonate of potash, and the mixture was exposed to a strong red heat, in a crucible of silver, during three hours.

When cold, the mass was softened with water, and was put into a glass matrass. I then added three ounces of pure con-

centrated muriatic acid, and digested it for two hours in a strong sand heat. Boiling water was then added, and the whole being poured on a filter, the residuum was edulcorated, dried, and made red-hot; it then weighed 85.50 grains.

C. The filtrated solution was evaporated to one fourth, and pure ammoniac being added, a precipitate was formed, which, after a red heat, weighed 10.70 grains.

D. One ounce of muriatic acid was poured on the 10.70 grains, in a matrass, which was then heated. The whole of the 10.70 grains was dissolved, excepting a small portion of siliceous earth, which weighed 0.30 grain.

E. The muriatic solution was then reduced by evaporation, to about one fourth; to which I added a large quantity of distilled water, which did not however produce any change. I then gradually added a solution of pure crystallized prussiate of potash, and heated the liquor till the whole of the iron was precipitated; after which, ammoniac precipitated a loose white earth, which, edulcorated and made red-hot, weighed 7.20 grains. The iron precipitated by the prussiate may therefore be estimated at 3.20 grains.

F. The 7.20 grains of the white earth were digested with sulphuric acid, and, after the excess of acid had been expelled by heat, boiling water was poured on the saline residuum. The solution was then gradually evaporated, with the addition of a small portion of lixivium of potash, and afforded crystals of alum, without a trace of any other substance.

G. I now proceeded to examine the 85.50 grains of B. These appeared to consist of siliceous earth, or fine particles of quartz, mingled with a considerable quantity of the dark grey shining particles.

Mr. Wedgwood was of opinion that these were a peculiar species of plumbago or graphite. Professor Blumenbach, on the contrary, regards them as molybdæna: and Mr. Klaproth believes them to be *eisenglimmer* or micaceous iron ore.

When rubbed between the fingers, they leave a dark grey stain, and the feel is unctuous, like that of plumbago, or molybdæna: the traces which they make on paper also resemble those of the abovementioned substances, but the lustre of the particles approaches nearer to that of molybdæna.

In order therefore to determine whether or not they consisted totally or partially of molybdæna, I put the 85.50 grains into a small glass retort, and added two ounces of concentrated nitric acid. The retort was then placed in a sand heat, and the distillation was continued, till the matter remained dry. The acid was then poured back into the retort, and distilled as before; but I did not observe that the grey particles had suffered any change, nor were nitrous fumes produced, as when molybdæna is thus treated.

To be more certain, however, I digested pure ammoniac on the residuum; and, having decanted it into a matrass, I evaporated it to dryness, without perceiving any vestige of oxide of molybdæna, or indeed of any other substance.

It was evident therefore that molybdæna was not present; and, as the general external characters and properties corresponded with those of plumbago, I was inclined to believe that these were particles of that substance, and not micaceous iron, as Mr. Klaproth imagined. To determine this, the following experiment was made.

H. 200 grains of pure nitre in powder were mixed with the 85.50 grains, and the mixture was gradually projected into a

crucible, made strongly red-hot. A feeble detonation took place at each projection; and, after a quarter of an hour had elapsed, the crucible was removed.

When cold, the mass was porous and white, without any appearance of the dark grey particles. Boiling water was poured on it, and the whole being put into a matrass, one ounce of muriatic acid was added, and digested with it in a sand heat. By evaporation it became gelatinous: it was then emptied on a filter, and, being well washed, dried, and made red-hot, weighed 75.25 grains.

The appearance of this was that of a white earth, arid to the touch. When melted with two parts of soda, a colourless glass was formed; and, with four parts of the same, it was soluble in water, and produced *liquor silicum;* it was therefore pure siliceous earth.

I. The filtrated liquor was saturated with ammoniac, and, upon being heated, a few brownish flocculi were precipitated, which, when collected and dried, weighed 0.40 grain. This precipitate was dissolved in muriatic acid, and was again precipitated by prussiate of potash, in the state of Prussian blue.

The liquor from which the flocculi of iron had been separated was then examined, by adding carbonate of potash, and lastly, by being evaporated to dryness; but it no longer afforded any earthy or metallic substance: so that, by the process of detonation with nitre, the 85.50 grains afforded 75.25 grains of pure siliceous earth, with 0.40 grain of iron; and, as the dark grey substance was destroyed, excepting the 0.40 grain of iron abovementioned, and as 9.85 grains of the original weight of 85.50 grains were dissipated, there can be no doubt but that this substance, amounting to 10.25 grains, was carburet of iron or

plumbago; especially as some experiments which I purposely made, on that from Keswick in Cumberland, were attended with similar results.

It is also evident, that these particles could not be *eisen-glimmer* or micaceous iron, as nitre has little or no effect on that substance, when projected into a heated crucible.

In a subsequent experiment on the same, the crucible was removed immediately after the last projection, and I then observed that an effervescence, with a disengagement of carbonic acid, took place, upon the addition of the muriatic acid, as is usual when pure plumbago is decomposed by nitre, and that less of the gelatinous matter was formed by evaporation.

The cause of this difference was evidently the duration of the red heat; for, in the first instance, the alkali developed by the decomposition of the nitre had time to unite with the siliceous earth, so as, when dissolved, to form *liquor silicum;* but, in the second experiment, a portion of alkali remained combined with the carbonic acid, produced by the carbon of the decomposed plumbago.

The produce of 100 grains by this analysis was,

		grains.
Silica - - -	D.	0.30
	H.	75.25
Alumine - - -	F.	7.20
Oxide of iron - -	E.	3.20
Graphite or plumbago -	I.	10.25
Water - - -	A.	2.20
		98.40

Mr. Wedgwood says, that sulphuric acid cannot dissolve the precipitated earth, and has but little effect on the mixed

substance, even when distilled to dryness; but, from the preceding experiments, I had reason to believe that the aluminous earth and iron would be separated by reiterated distillation; I therefore repeated the analysis in the following manner.

Second Analysis of the Sydneia, *No.* 2.

A. 100 grains of the earth were put into a glass retort, upon which 400 grains of pure concentrated sulphuric acid were poured. The retort was placed in a small reverberatory, and the fire was continued till a dry mass remained. 400 grains of the acid were again poured in, and distilled as before. Upon the dry mass, boiling water was poured, and the whole was then emptied on a filter, and edulcorated. The residuum, after a red heat, weighed 87.75 grains, and consisted of siliceous earth, mixed with some mica, and with particles of plumbago.

B. The filtrated solution, by ammoniac, afforded a precipitate, which weighed 9.50 grains; and, being examined, as in the former experiment, yielded 6.50 grains of alumine, and 3 grains of oxide of iron.

The plumbago was separated from the siliceous matter, in the manner already described, and amounted to about 10 grains.

By this analysis I obtained,

	grains.
Silica and mica - -	77.75
Alumine - - -	6.50
Oxide of iron - - -	3
Plumbago - - -	10
	97.25

It appears therefore that the Sydneian earth, when treated with sulphuric acid, is capable of being for the greater part

decomposed; and Mr. Wedgwood probably did not succeed, because his process was in some respect different, or that the distillation was not sufficiently repeated.

I have not thought it necessary to be more circumstantial in the account of this second analysis, as the operations were similar to those of the former.

§. 4.

These experiments prove, that the earthy substance called *Sydneia* or *terra australis*, consists of siliceous earth, alumine, oxide of iron, and black lead or graphite.

The presence of the latter appears to be accidental, and it probably was mixed with the other substances at the time when they were transported, and deposited, by means of water; for this appears evidently to have been the case, from the general characters of this mixed earthy substance.

The quartz and mica, which are so visible, indicate a granitic origin; and the soft white earth has probably been formed by a decomposition of feldt spar, such as is to be seen in many places, and particularly at St. Stephen's, in Cornwall. The granitic sand which covers the borders of the *Mer de Glace*, at Chamouni, in Savoy, also much resembles the *terra australis*, excepting that the feldt spar is not in a state of decomposition: in short, the general aspect, and the analysis, concur to prove, that the *Sydneia* has been formed by the disintegration and decomposition of granite, or *gneiss*.

Mr. Wedgwood's experiments are so circumstantial, that had I only examined the earth last brought to England, I should have supposed, with Mr. Nicholson, that I had operated on a different substance; but, as I had an opportunity to examine,

by analysis, a portion of the same earth on which Mr. WEDGWOOD made his experiments, and as I received it from Sir JOSEPH BANKS, the same gentleman who had furnished Mr. WEDGWOOD with it, no suspicion can be entertained about its identity.

Some of the experiments which I have related, and which prove that some of the finer earthy particles remained suspended in the concentrated muriatic acid, and were precipitated when the acid was diluted with water, appear in some measure to account for the mistake which has been made, in supposing that a primitive earth, before unknown, was present; but this alone will not account for many of the other properties mentioned by Mr. WEDGWOOD, such as,

1st. The repeated and exclusive solubility in the muriatic acid, and subsequent precipitation by water.

2dly. The butyraceous mass which was formed by evaporation.

And, 3dly. The degree of fusibility of the precipitated earth.

These indeed I can by no means explain, but by supposing that the acids used by Mr. WEDGWOOD were impure. This supposition appears to be corroborated by a passage in Mr. WEDGWOOD's paper, where he says, "here the Prussian lixi-"vium, in whatever quantity it was added, occasioned no pre-"cipitation at all, (only the usual bluishness arising from the "iron always found in *the common acids*.")* Now if (as it seems from this expression) Mr. WEDGWOOD employed the common acids of the shops, without having previously examined and purified them, all certainty of analysis must fall, as the impurity of such acids is well known to every practical

* Philosophical Transactions, Vol. LXXX. Part II. p. 313.

chemist; but, whether this was the cause, or not, of the effects described by Mr. Wedgwood, I do not hesitate to assert, that the mineral which has been examined does not contain any primitive earth, or substance possessing the properties ascribed to it, and consequently, that the Sydneian genus, in future, must be omitted in the mineral system.

VII. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, *for the Year* 1796. *By* Thomas Barker, *Esq. Communicated by Mr.* Timothy Lane, *F. R. S.*

Read February 15, 1798.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest.	Lowest.	Mean.	High.	Low.	Mean.	High.	Low.	Mean.	
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inches.
Jan.	Morn.	29,77	28,47	29,17	50	41	45	52½	32	42½	1,955
	Aftern.				52	41½	46	55	34	47	
Feb.	Morn.	29,87	28,53	32	45½	38	42	49	31½	38	1,643
	Aftern.				51	34	42½	51½	32	43	
Mar.	Morn.	29,97	29,22	57	48	33	41	45	26	36	0,376
	Aftern.				51	34	42½	57	34	45	
Apr.	Morn.	29,88	28,67	59	59	44	50½	52	36	45	0,649
	Aftern.				61	45	52	72	45	57½	
May	Morn.	29,73	28,33	27	55	47	51	52	40	46	2,839
	Aftern.				56	48	52	65	48	56	
June	Morn.	29,84	29,05	47	59	50½	55	60	46	54	0,927
	Aftern.				70	52	57	80	56	67	
July	Morn.	29,73	28,91	32	67	50	58½	64½	49½	56½	5,646
	Aftern.				67½	54½	60½	77	59	68½	
Aug.	Morn.	29,95	29,23	60	65½	57	61	62½	49	56	1,120
	Aftern.				68½	58½	63	77	57	69	
Sep.	Morn.	29,83	29,03	50	65	53	59½	63	42½	54	1,891
	Aftern.				68	54	61	77	56	66	
Oct.	Morn.	30,07	28,75	45	55½	43½	49	59	29	43	1,320
	Aftern.				56	44½	50½	61½	41	51½	
Nov.	Morn.	29,86	28,69	36	51½	39½	43	52½	26	38	2,048
	Aftern.				53	39	43½	57	28½	43	
Dec.	Morn.	29,98	28,75	31	44½	28	34	48	14½	30	1,668
	Aftern.				46½	29½	35	49	25	34	
											22,082

THE year began with a remarkably open winter, sometimes quite warm and pleasant, and several times thunder. It was showery at first, then dry and fine; but the end of January and beginning of February were wet, yet still open and mild, and more dry afterward; but colder, and inclined to frost, the end of February, and in March, the middle of which was again mild and fine, and not windy, but frosty toward the end. The last days of March, and beginning of April, were dry and pleasant; a good seed time, and calm; but rain was wanted toward the end, which came plentifully the end of April, and beginning of May. The season in general cool, and the latter half more dry; too much so in the south of England, for grass and hay were scarce there; but, in this country, both grass and corn came on well, and continued to do so all June, which was of a moderate heat, with a mixture of wet and dry; frequent but moderate winds, and calm at the end. There was plenty of hay this year; but, through a very wet and windy July, a good deal of it was not well got. The crops of grain were almost all good, and the moist July made the beans and pease remarkably so. The harvest, though threatening at first, was in general very well got; the weather being chiefly fair, and rather hot, with some rain at times, kept the grass in a growing state, of which there was plenty left upon the ground against winter.

The autumn was in general very fine and pleasant; for the most part fair, with few frosty mornings, till near the end of November; when a severer season began, and continued very hard frost the first third of December; then an imperfect break for some days, but not so as to take the frost all away. It returned again as hard as before, and continued another third

number the vibrations without affecting them, I dropped the idea for that time. I learnt, however, some time afterwards, that Mr. JOHN WHITEHURST, a very ingenious person, had been in pursuit of the same object with better success, and had contrived a machine fully corresponding to his expectations and my wishes. This he afterwards explained to the world, in a pamphlet, entitled, "An Attempt to obtain Measures of Length, "&c. from the Mensuration of Time, or the true Length of "Pendulums;" published in 1787. Mr. WHITEHURST having therein done all that related to the standard measure of length, and suggested that of weight, it appeared to me that it remained only to verify and complete his experiments.

(§. 3.) For this purpose, by the kind assistance of my friend Dr. G. FORDYCE, who, at Mr. WHITEHURST'S death, had purchased his apparatus, I was furnished with the very machine with which Mr. WHITEHURST had made his observations. I also procured to be made, by Mr. TROUGHTON, a very excellent beam-compass or divided scale, furnished with microscopes and micrometer, for the most exact observations of longitudinal measure: as also a very nice beam or hydrostatic balance, sensible with the $\frac{1}{100}$ of a grain, when loaded with 6lb. Troy at each end. Mr. ARNOLD made me one of his admirable time-keepers, in order to carry time from my sidereal regulator in my observatory, with which it was adjusted, to the room wherein I had fixed Mr. WHITEHURST'S pendulum; and who, having taken a journey from London into Warwickshire, was so good as to assist in the beginning of these experiments. Thus equipped, I went to work in the latter end of August, 1796, when the temperature was about 60°, first to examine the length of the pendulum; when, to my great mortification, I found that the

thin wire, of which the rod consisted, was too weak to support the ball in a state of vibration; and that, after 15 or 20 hours action, it repeatedly broke. The same misfortune attended my trials with three other different sorts of wires that I had obtained from London. Whether this accident happened from any rust in the old wire, or from want of due temper in the new, or from its being too much pinched between the cheeks,* I cannot tell: I can only observe, that all the wires that I used were considerably heavier, and therefore probably stronger, than what Mr. WHITEHURST mentions, *viz.* 3 grains in weight for 80 inches in length; nay, mine proceeded as far as from 5 to 6 grains for that length, and yet I could never get it to support the ball during the whole period of my experiment. This being the case, and being in the country, far removed from the manufactory of this fine wire, I was reluctantly compelled to relinquish this part of the operation to some more favourable opportunity. In the mean while, however, I thought it desirable to measure the difference of the lengths of Mr. WHITEHURST'S pendulum from his own observations; for, very fortunately, the marks that he had made on the brass vertical ruler of his machine were still visible; and this interval, which he calls "59,892 inches," I determined, on my divided scale made by TROUGHTON, from Mr. BIRD'S standard, to be = 59,89358 inches, from a mean of four different trials in the temperature of 64°; that mean differing from the extremes only = ,0003 inch.

(§. 4.) By this examination, if I have not verified, I have at least preserved, Mr. WHITEHURST'S standard; and, for the present, I shall consider this measure of the difference of the length of the two pendulums, vibrating 42 and 84 times in a

* C, C, fig. 1. of Plate II. in Mr. WHITEHURST'S pamphlet.

minute of mean time, as correct. On this presumption, I shall proceed to the examination of weight.

(§. 5.) From the opinion of different skilful persons, with whom I have conferred, as well as from the result of my own considerations, I am inclined to believe there is hardly any body in nature, with which we are familiarly acquainted, that is of so simple and homogeneous a quality as pure distilled water, or so fit for the purposes of this inquiry: and I have concluded, that if the weight of any quantity of water, whose bulk had been previously measured by the abovementioned scale, could be obtained, under a known pressure* and temperature of the atmosphere, we should be in possession of a general standard of weight.

(§. 6.) With this view, I directed Mr. Troughton to make, in addition to the very sensible hydrostatic balance before mentioned, a solid cube of brass, whose sides were 5 inches; and also a cylinder of the same metal, 4 inches in diameter, and 6 high. From St. Thomas's hospital, by the favour of Dr. Fordyce, I procured 3 gallons of distilled water. With these I made the following observations; but, before I relate the experiments, I will describe the apparatus.

Mr. Whitehurst's machine for measuring the pendulum has been sufficiently explained in his pamphlet mentioned above; my divided scale, which was a new instrument, was as follows.

* I do not here mean to infer any opinion respecting the compressibility of water; but only to say, that where water, or any thing else, is weighed in air, the density of that medium, as shewn by the barometer and thermometer, must be known, in order to make allowances for it, if necessary.

(§. 7.) *Description of the Beam Compass, or divided Scale of equal Parts.*

a b, (Tab. V. fig. 1.) is a block or beam of mahogany, 6 feet 3 inches long, 6 inches deep, and 5 wide, upon which are laid two brass rulers, *c d e*, and *f g*, each divided into 60 inches, and tenths. The former of these, called the Scale, is, for a time, kept immoveable by the finger-screws *c e d*, and is furnished with very fine hair-line divisions, intended to be viewed only by the microscopes *h, i*: the latter, called the Beam, has no motion but by means of the screw *g*, and bears stronger divisions upon it, with which the sliding pieces or indexes, at *k* and *m*, may readily be compared by the naked eye, and is intended only to set the microscopes, or rather the wires, in their focus, to the required distance nearly, *viz.* to within $\frac{1}{100}$ or $\frac{1}{200}$ of an inch. The microscopes are compound, and similar to those described by the late General Roy, in his account of his large theodolite. (See Phil. Trans. Vol. LXXX.) The one at *h*, contains only cross wires fixed in its focus; the other at *i*, has a micrometer also, by means of which its cross wires may be moved to the right or left, over the image of the divisions of the scale, any given space, not exceeding $\frac{1}{10}$ inch; and the quantity so moved may be measured by the divisions on the screw head, passing under the index at *o*. The divisions on these rules have been called inches and tenths: it was not necessary that they should be more than equal parts; but they were in fact laid down by Mr. Troughton, from a scale of the late excellent artist Mr. J. Bird, who had divided into inches several scales of different lengths; one of which, 42 inches long, belonged to the late General Roy; a second, of 5 feet, was purchased by Alexander Aubert, Esq. and a third, of

90 inches, which is now the property of the Royal Society, is kept in their archives, and is said to have been used by Mr. Bird, in dividing his large mural quadrants.* Besides these, he made two standards of three feet, by order of the House of Commons, of which I shall speak more hereafter. The mode of using this instrument is as follows.

(§. 8.) Let the object to be measured be supposed to be about six inches, and let it be desired to compare it with the interval between the 20th and the 26th division on the scale *c d*: move by hand the microscope *b*, with its sliding plate, until the division of the index at *k* coincide with the division of 20 inches on the rule *f g*; then move by hand also the microscope *i*, with its sliding plate and appendage *l m n o*, until the index division near *m* coincide with 26 inches on *f g*: the axes of the microscopes, or centres of their cross wires, will be at the approximate distance of 6 inches. To correct this, examine if the wires of *b* correspond with a division on *c d*; if not, move the rule *f g* backward or forward, by the screw *g*, till they do, then will the microscope *b* be adjusted. Now examine if the wires in *i* cover exactly a division; if they do so, the true interval of 6 inches between the microscopes is obtained; if not, move the microscope *i* a little, by means of the screw *l*, till they do, and both the microscopes will be adjusted: then remove the rule *c e d* from its place, by taking out the screws *c e d*, and place the object to be measured in its room, at the same time taking care that it be exactly in the focus of the object glass of the microscope, in such manner that one extremity may correspond with the wires in the microscope *b*; that done, if the other extremity coincide with the wires in *i*, the dimension of the object is exactly 6 inches; if not, restore the coincidence,

* A farther account of these scales is given in the Appendix.

by turning the micrometer screw *n*, and the divisions at *o* will give the difference, in 1000ths and 10,000ths of an inch, + or — 6 inches.

(§. 9.) *Description of the Hydrostatic Balance.*

a b c d, (Tab. VI.) is a box, which contains the whole apparatus when not in use, and when used serves as a foot to the hollow brass pillar *e f g h*, which is fixed into it by the four screws at the bottom *e* and *f*. This pillar contains another within it, which is raised up and down about $\frac{1}{10}$ inch, by means of the screw *x*. *n o* is the beam, 27 inches long, and 3,9 inches wide in its greatest diameter; each arm of which is made hollow and conical, for strength and lightness: through the centre, at *m*, passes the axis of motion, the ends of which, when used, are suffered to fall gently upon two crystal planes, which are set horizontally by means of the spirit levels *k*, *l*, and the screws underneath the box, at *c* and *b*. The ends of this axis are of hardened steel, of a wedge-like shape, and reduced to a fine edge, *viz.* to an angle of about 40°, so as to move upon the planes with very little friction, and at the same time so hard as (with due care in using) to be in no danger of being blunted: to prevent which, the inner pillar has a motion upwards, as has been said, by the screw *x*, and, by means of a semicircular arm at its upper extremity, lifts the beam off its bearings, when it is not used, or is greatly loaded. This axis is placed carefully at right angles to the beam; and, by means of two small brass springs that press gently at the ends, is brought always to have the same bearing upon the crystal; so that no error need be feared from a small deviation from the right-angular position of the axis to the beam, should any such

exist; and, from its shape and quality, it may be considered as inflexible in any ordinary experiments. At *p* is a small adjusting screw, which raises or depresses a weight within, and with it, in consequence, the centre of gravity of the whole beam; by this means, the motion on its centre may be brought to almost any required degree of sensibility. Should the centre of gravity be raised above the centre of motion, the beam would turn over; if it be in that centre, the beam would stand any where indifferently, without any vibration; if it be placed much below it, the vibration would be too quick, and its sensibility not sufficient: it is therefore brought, by the screw *p*, a very small quantity below the centre of motion, so as to describe one vibration in 40 or 50 seconds; the sensibility is then fully sufficient. At each end of the beam are circular boxes, *n* and *o*, through which pass the steel centres, from whence are suspended the scale-pans *q* and *r*: these centres resemble, in some degree, those at *m*, but have their chamfered or angular edges upwards, and thereon hang the hooks β, to which are affixed the links α, and to them the three silken lines of the scale. Each of these centres has a motion in its respective box, by means of two small adjusting screws; that in *o* laterally, and that in *n* vertically; the former to make the two arms of the beam of an equal length, the latter to bring the three points of suspension of the beam and scales into a right line. At the extremity of the boxes are fixed two needle points or indexes, which play against the ivory scale of divisions at *s* and *t*. These divisions, although they do not, indeed they cannot, shew any definite weight, are nevertheless very useful in making the adjustments, and even in weighing to the small fractions of a grain. *u v* are two steady plates, that are raised or depressed by the wooden nut

w, to check the vibrations of the scales *q* and *r*, and bring them more speedily to an equilibrium. *y z* is a table, whereon the whole is placed, to raise it to a height convenient for experiments.

To use with this beam, I had three sets of weights made, *viz.*

The 1st set or series of 15 weights, rising in a duplicate progression from 1 to 16384 grains, *viz.*

No.		Grains.
1 =	-	1
2 =	-	2
3 =	-	4
4 =	-	8
5 =	-	16
6 =	-	32
7 =	-	64
8 =	-	128
9 =	-	256
10 =	-	512
11 =	-	1024
12 =	-	2048
13 =	-	4096
14 =	-	8192
15 =	-	16384

Fractions of a Grain.
$\frac{1}{32}$
$\frac{1}{16}$
$\frac{1}{8}$
$\frac{1}{4}$
$\frac{1}{2}$

The 2d series of weights, in an arithmetical order, as follow, *viz.*

Grains.	Grains.	Grains.	Grains.	Decimal Fractions of a Grain, *viz.* 100th Grain.	Tenths.
1	10	100	1000	,01	,10
2	20	200	2000	,02	,20
3	30	. . .	. . . .	,03	,30
4	40	400	4000	,04	,40
5	50	. . .	. . . .	,05	,50
6	60	600	6000	,06	,60
7	70	. . .	. . . .	,07	,70
8	80	800	8000	,08	,80
9	90	. . .	. . .	,09	,90
			10,000		
			20,000		

N. B. The fractions of a grain are made of fine wire flatted.

The 3d set consists of a weight of { 1 ounce, 2 ounces, 4 ounces, 8 ounces, 1 pound } Troy.

(§. 10.) *a b c d* and *a b c d*, (Tab. VII. fig. 1.) is the brass cube of 5 inches that has been mentioned, suspended in its own scale, by means of four fine wires, from the arm *o* of the beam, Tab. VI. by taking away the common scale *α r*. The cube rests upon a cradle or cross, three arms of which are seen at *g h i*, and by this means may be weighed either in air or water, by immersion into the large glass vessel *g h*, Tab. VII. fig. 3.

At fig. 2. is seen the cylinder *a b c d* and *a b c d*, four inches in diameter, and five high, slung in another cradle, part of which is seen at *g b h i*, supported by four wires from the point *f*.

In fig. 3. is seen a sphere of brass *d*, 6 inches in diameter, slung in a cradle *a b c*, by three wires* from the links *f*, suspended in a glass jar,† containing near four gallons of water, whose temperature is shewn by a thermometer at *e*.

* These wires were of such a size that 91 inches weighed 20,71 grains, consequently 1 inch = 0,2276 grain, and the three wires = 0,6828 grain; and their specific gravity being 8,7, their loss of weight, by sinking 1 inch in water, would be = 0,0785 grain. This correction it may be necessary hereafter to attend to.

† The glass jar is made somewhat conical, being in

		inches.
Diameter at top	- - -	12,0
Ditto at bottom	- - -	8,7
Mean ditto	- - - -	10,35
Mean height within	- -	11,8
Contents in cubic inches	-	= 992,78
Which is in ale gallons	- -	= 3,8 = 15⅕ quarts.

It may also be noted, that 1 inch in depth of the water near the top is = 113 cubic inches, which is equal to the exact bulk of the sphere, as will be seen hereafter.

(§. 11.) It was necessary to measure the exact size, and correctness of figure, of this sphere. For this purpose was made a wooden gauge or frame *a b c d e*, (Tab. V. fig. 2.) in which the sphere was placed, upon semicircular pieces within, lined with green cloth to prevent bruising it: upon this frame was placed a brass square *k l m n*, whose sides were about $\frac{1}{100}$ inch in length more than the diameter of the sphere. This square, by raising or lowering the screws *o r s*, was easily made to coincide with a plane passing through the centre of the sphere. *p* is a micrometer screw, the interior extremity of which is brought just to touch the surface of the sphere, while the opposite side bears gently against the interior side of the frame at *o*; and, by turning the sphere round, so as to present different diameters to these points of contact, any variety in the diameter may be seen by the index *l*, and plate *q*, divided into 10,000ths inch. To render this operation more convenient, three great circles were drawn with a pencil upon the sphere, at 90° distance from each other, (the two former were traced by the artist in the lathe, while the sphere was making, and the third was drawn from them,) and each was divided into 8 equal parts. The immediate result of these experiments would only give the differences, and not the absolute quantity, of the diameter; for this purpose, a brass ruler *r*, fig. 3. was made, of such a length as just to go within the brass frame *k l m n*; and, being substituted in the place of the sphere, could easily be compared with any given diameter, and afterwards measured with the divided scale, fig. 1. With these instruments I made the following observations, August 31, 1796, the thermometer being at 61°.

(§. 12.) *Examination of the Dimensions of the Brass Cube, by Means of the divided Scale.*

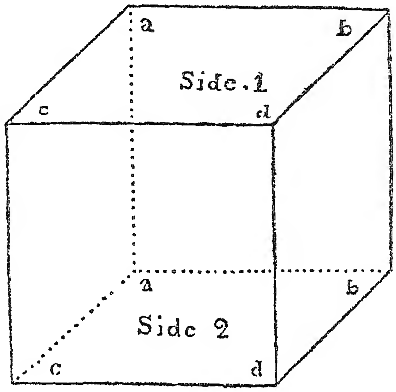
The microscope and micrometer being both adjusted, as well with respect to their focus,* as to the value of the micrometer scale, the cross wires in their focus were removed to a distance from each other of five inches *nearly* on the beam, (the former being at 27, and the latter at 32, inches,) and then *correctly* adjusted to this interval on the divided scale. I must observe, indeed, that the value of the micrometer scale was not exactly ten revolutions of the screw to $\frac{1}{10}$ inch, as Mr. TROUGHTON designed; but this measure by the screw,† from 6 trials, was deficient by — 0,0002 inch; *viz.* two ten-thousandths of an inch were to be added to each tenth of an inch measured by the micrometer, and so in proportion for a less quantity; but this correction is hardly worth notice.

	On the scale.		
	inch.	inch.	inches.
The interval of the cross wires in the microscope and micrometer -	27 and	32 =	5,0000
Interval of ditto, on another part of the scale, *viz.* - - -	26 and	31 =	5,0000 —
Ditto, ditto - - - -	25 and	30 =	5,0001 +

		Inch.	
* The focal length of the object lens is - - -	=	0,75	
The distance of the cross wires from the object lens	=	2,00	
The focal length of the combined eye glass -	=	1,50	
Whence the magnifying power of the microscope becomes	=	14,2	times.
† One Revolution of the screw of the micrometer was -	=	$\frac{1}{100}$	inch.
Each grand division, of which there were ten -	=	$\frac{1}{1000}$	inch.
These again subdivided into five, each became -	=	$\frac{1}{5000}$	inch.
And half a division, which is very visible, is -	=	$\frac{1}{10000}$	inch.

I therefore say, this interval was 5 inches correctly, to within less than the twenty thousandth part of an inch, on this scale.

Measurement of the Cube, viz. *of the Side* 1. (*See the Figure.*)



$$\begin{array}{rl} & \text{Inches.} \\ \text{From } a \text{ to } b = & 5 - ,0114 \\ a \text{ to } c = & 5 - ,0115 \\ c \text{ to } d = & 5 - ,0105 \\ b \text{ to } d = & 5 - ,0113 \end{array} \begin{array}{l} \\ \text{therefore} \\ - \quad - \\ - \\ - \quad - \end{array} \left.\begin{array}{l} \text{Inches.} \\ = 4,9886 \\ = 4,9885 \\ = 4,9895 \\ = 4,9887 \end{array}\right\} \begin{array}{l} \text{Mean.} \\ \text{Inches.} \\ = 4,98882 \end{array}$$

The Side 2.

$$\begin{array}{rl} \text{From } a \text{ to } b = & 5 - ,0106 \quad - \quad - \\ a \text{ to } c = & 5 - ,0098 \quad - \\ c \text{ to } d = & 5 - ,0102 \quad - \quad - \\ b \text{ to } d = & 5 - ,0112 \quad - \end{array} \left.\begin{array}{l} = 4,9894 \\ = 4,9902 \\ = 4,9898 \\ = 4,9888 \end{array}\right\} = 4,98955$$

Height of the Cube, from Side 1 *to Side* 2.

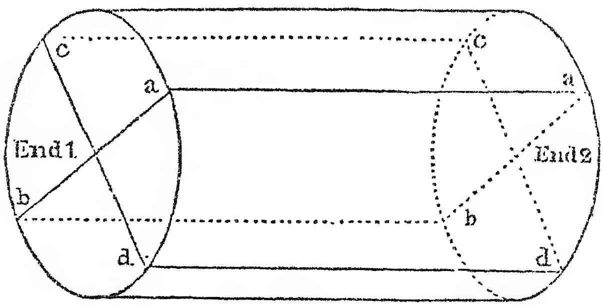
$$\begin{array}{rl} & \text{Inches.} \\ \text{From } a \text{ to } a = & 5 - ,0110 \quad - \quad - \\ b \text{ to } b = & 5 - ,0105 \quad - \\ c \text{ to } c = & 5 - ,0107 \quad - \quad - \\ d \text{ to } d = & 5 - ,0108 \quad - \end{array} \left.\begin{array}{l} \text{Inches.} \\ = 4,9890 \\ = 4,9895 \\ = 4,9893 \\ = 4,9892 \end{array}\right\} = 4,98925^*$$

* It cannot escape notice, that all these measures were something less than 5 inches, the quantity proposed: it arose from this, Mr. TROUGHTON informs me, he was more

(§. 13.) Now the three foregoing mean measures of the side of the cube, multiplied into each other, will give = 124,18917 cubic inches, for the contents of the brass cube; which must be very near the truth; for, if not, let us suppose the error, in taking each of these measurements, to be half a thousandth of an inch, which is much greater than is probable, *viz.* = $\frac{1}{10,000}$ part of the side of the cube; and let us suppose each of these errors to lie the same way, which is also very improbable; in that case, the error in determining the solid content would be only $\frac{3}{10,000}$ of the whole; in the above instance, about 0,03 cubic inch; but, more probably, the error does not amount to half this quantity.

(§. 14.) *Examination of the Cylinder.*

The micrometer and microscope of the divided scale (Tab. V. fig. 1.) being removed till their cross wires were four inches distant, *viz.* from 54 inches to 58 inches, and the thermometer at 62°, I observed, of the end or base of the cylinder, No. 1.



solicitous to obtain a true figure, than the exact size; neither of which however were very important, as both were to be proved by the mode I have adopted. What was important, was to have the sides true planes; and these were examined, as I am informed, by the reflected image of the moon, seen through a large telescope, the focus of which would be altered, if the surface were either hollow or convex.

$$\begin{array}{lll} & \text{Inches.} & \text{Inches.} & \text{Mean. Inches.} \\ \text{The diameter } a\,b = & 4 - ,0027 = & 3,9973 & \multirow{2}{*}{= 3,99745} \\ c\,d = & 4 - ,0024 = & 3,9976 & \end{array}$$

End 2. of the Cylinder.

$$\begin{array}{lll} & \text{Inches.} & \text{Inches.} & \\ \text{The diameter } a\,b = & 4 - ,0014 = & 3,9986 & \multirow{2}{*}{= 3,99785} \\ c\,d = & 4 - ,0029 = & 3,9971 & \end{array}$$

Height of the Cylinder.

The microscope and micrometer being placed respectively at 52,1 inches and 58,1 inches, *viz.* at the interval of exactly 6 inches on the scale, I found

$$\begin{array}{llll} & \text{Inches.} & \text{Inches.} & \text{Mean.} \\ \text{The height from } a \text{ to } a = & 6 - ,0049 = & 5,9951 & \\ b \text{ to } b = & 6 - ,0047 = & 5,9953 & \\ c \text{ to } c = & 6 - ,0047 = & 5,9953 & \text{Inches.} = 5,99502 \\ d \text{ to } d = & 6 - ,0054 & & \\ \text{Repeated} - & \left\{ \begin{array}{l} 58 \\ 56 \end{array} \right\} & = 5,9944 & \end{array}$$

Now the mean diameter of the cylinder having been found

$$\begin{array}{ll} & \text{Inches.} \\ \text{at the end } 1 = & 3,99745 \\ \text{at the end } 2 = & 3,99785 \end{array}$$

The factor for the square of the diameter of a circle, to find the area, being, as is well known, $\}$ = 0,7854

And the height of the cylinder = 5,9950

The above four quantities, multiplied into each other, give for the contents of this cylinder, in inches, = 74,94823; and this result may be taken at least as correct as that of the cube, *viz.* to about the third place of decimals.

(§. 15.) Having adjusted the beam of the balance, (Tab. VI.) with respect to the length of its arms, its centre of gravity, and the three points of suspension of the beam and scales, and having examined the weights, I proceeded to the remaining parts of this experiment.

Sept. 2d, 1796. The balance beam adjusted by the screw *p*, till the vibrations were so slow as to require more than 50 seconds of time for each, $\frac{1}{100}$ grain appeared to move the index through three divisions* of the scale *s* and *t*, $= \frac{1}{7}$ inch, when the beam was not loaded; but, when the beam was loaded with 16384 grains, or near 3lb. Troy, $\frac{1}{100}$ grain was equal only to $0\frac{1}{2}$ division† of the same scale.

(§. 16.) Sept. 4th. The thermometer being at 63°, and the barometer at 29,36 inches.

	oz.	grains.		grains.
The weight of the counterpoise to the pan or scale for weighing the cube in air, was - - -	= 1	75,02	=	555,02

* 20 divisions are = 1,0 inch.

† That is, the beam was sensible with $\frac{1}{1638400}$ part of the whole weight. Mr. HARRIS's beam, with which he and Mr. BIRD made their observations on the Exchequer weights, turned with $\frac{1}{230000}$ part of the whole weight, and was consequently only $\frac{1}{7}$ part so sensible as this. See "The Report of the Committee of the House of "Commons in 1758, to inquire into the original Standards of Weights and Measures "in this Kingdom, and to consider the Laws relating thereto." See also a second Report in 1759; both of which contain a vast deal of useful information on this subject, extending through fifty folio pages, and are to be found in the 2d volume of Reports, from 1737 to 1767. A bill was brought in, in consequence, but afterwards dropped; and it is much to be lamented, that this inquiry did not go to the full length of an act of parliament. Note farther, the largest of the beams, of which there are four of different sizes, now made use of in the dutchy court of Lancaster, for the actual sizing of the weights of the kingdom, is about 3 feet long, and is moveable with about 30 grains, when 56lb. avoirdupois are in each scale, *viz.* about $\frac{1}{13000}$ part of the whole.

	oz.	grs.	grains.
To which, add the weight of the common pan with the silk lines, on the left arm of the beam, and marked with ×, the common right-hand pan having been removed - - - -	-	-	= 413,40
And the whole weight of the pan or apparatus for weighing the *cube in air*, becomes - - - -	-	-	= 968,42
(§. 17.) The counterpoise to the pan or scale for weighing the cylinder in air, was found - - - -	= 1	72,34	= 552,34
To which, add the weight of the common pan on the left arm, as before	-	-	= 413,40
And the whole weight of the pan or scale for weighing the *cylinder in air*, becomes - - - -	-	-	= 965,74

Note, in the preceding and such like experiments, the common right-hand scale being removed, and the left-hand scale being always used, and always the same weight, *viz.* 413,40 grains, when either the cube, or cylinder, or any large body, is weighed, notice need only be taken of the counterpoise weights, *viz.* 555,02 grains, or 552,34 grains, respectively; and these are to be deducted from the general amount of all the weights in the left-hand scale, marked ×; but it certainly would have been more convenient to have had single weights, ready adjusted, for these counterpoises, both in air and in water. These, though at first omitted, have since been supplied.

	grains.
(§. 18.) The counterpoise to the scale for the cube, in distilled water, with the heat of 61° -	= 442,75
To this, add the weight of the common scale, as before	= 413,40

And we have the whole weight of the scale for the cube in water - - - - -	=	grains. 856,15
But the weight in air having already been found -	=	968,40
The difference of the weights - - -	=	112,25
Gives for the specific gravity of this brass -	=	8,62
(§. 19.) The counterpoise to the scale for the cylinder, in the same water, with the same heat -	=	441,68
To this, add the weight of the common scale, as before	=	413,40
And the whole weight of the scale for the cylinder, in water, becomes - - - -	=	855,08
Its weight in air has already been found - -	=	965,74
The difference of these weights - - -	=	110,66
Gives for the specific gravity of this brass -	=	8,78
The mean specific gravity of this brass and brass-wire may therefore be put at about - -	=	8,7

N. B. The tables of specific gravity give that of wrought brass from 8,00 to 8,20. It was necessary to ascertain the specific gravity of the brass wire, to make the correction mentioned in the note to §. 10.; for, as it was highly probable, that in experiments with this hydrostatic balance, the scales for the cube and cylinder would occasionally be immersed to different depths in the water, and their weights would be altered, as more or less of the wires by which they were suspended remained out of the water;

I accordingly found, that 80 inches in length of this wire, used in the scales for the cube and cylinder, weighed in air - - - -	=	grains. 6,16

And consequently, 1 inch would be = 0,077 grain, and four wires of 1 inch = ,308 grain; which, divided by the specific gravity, *viz.* $\frac{308}{8,7}$, would give 0,0354 grain, for the correction of

every inch that the scale was sunk lower in the water; and so in proportion.

(§. 20.) *Experiment of the Cube of Brass weighed in Air.*

The cube was suspended to the right arm of the beam, by the scale belonging to it, and the left scale pan, with the mark ×, was hung at the other end of the beam, in which were placed the following weights,* made by E. TROUGHTON.

	grains.
viz. No. 15 of	16384
14 -	8192
13 -	4096
12 -	2048
11 -	1024
9 -	256
	84,82

The total weight of the cube in air = 32084,82 { the barom. being at 29,0 Inch.; the therm. - at 62°,0

(§. 21.) *Experiment of the Weight of the Cylinder in Air.*

	grains.
No. 15 of	16384
13 -	4096
11 -	1024
	53,37
	21,557,37

But a counterpoise of 555,02 grains. having been used, by mistake, instead of - 552,34

Add this excess = 2,68 } = + 2,68

And the total weight of the cylinder is - - } = 21,560,05 { the barom. at 29,0 inch.; the therm. at 62°,0

* This scale contained also 555,02 grains, being the weight or counterpoise to the scale for the cube.

(§. 22.) *The Cube weighed in distilled Water.*

		grains.
Sept. 5. Put into the left scale, the counterpoise for the water scale - -	300, 100	= 400,00
The cube, with its scale, was then immersed in the water.		
I then restored the equilibrium, by putting into the opposite or left-hand common scale, Mr. TROUGHTON's weights, No. 10. - - -		grains. = 512,00
(The barom. standing at 29,47 inches, the therm. at 60°,2.)		200,
		30,
		3,70
		745,70
But a counterpoise of - -	grains. 400	
having been taken, by mistake, instead of	442,75	= − 42,75
Deduct the difference, which was so left out =	42,75	
The apparent weight of the cube in water becomes =		702,95
Add the correction* for the loss of weight of the 4 wires, by immersion $2\frac{1}{4}$ inches deeper than when the counterpoise was adjusted -		= + ,08
And the true corrected weight of the cube in water, with 60°,2 of heat, becomes - -		703,03

* When the cube was immersed, the water in the glass jar stood $2\frac{1}{4}$ inches higher than when the counterpoise for this water-scale was adjusted, and found to be 442,75 grains; (see §. 18.) and 1 inch of alteration in the height of the water having appeared to be = 0,0354 grain in weight, (§. 19) $2\frac{1}{4}$ inches will be = 0,078 grain; and so much must be added, to correct for the loss of weight, in the four wires, that suspended the scale and cube in water. When the cube was immersed, the surface of the water stood 1,5 inch below the top of the glass jar, and 9,7 inch below the centre of the beam, or index point.

When the cube was in the water, the beam was clearly sensible with $\frac{1}{10}$ of a grain.

(§. 23.) *Experiment of the Cylinder in distilled Water.*

Sept. 5. The thermometer being at from 60°,2 to 60°,5, and the barometer 29,47 inches,

	grains.	
Put into the left scale pan, the counterpoise to the water-scale for the cylinder -	300	
	100	= 441,7
	41,7	

The cylinder, with its water-scale, was immersed in water. I then restored the equilibrium, by putting into the left scale,

		grains.
Mr. TROUGHTON's weights, No. 12	-	= 2048
No. 9	-	= 256
		200
		30
		10
		4
		1,10
Weight of the cylinder in water	- -	= 2549,10
Add the correction for the loss of weight of the four wires, by being $1\frac{1}{2}$ inch deeper immersed in the water, than when the counterpoise was adjusted - -		= + 0,05*
Corrected weight of the cylinder in water	-	= 2549,15

* In order that *this* and some other corrections may be the more easily applied, I have computed the 3 following tables, to be used whenever great accuracy is required.

After this experiment, I discovered that some small bubbles of air had insinuated themselves between the cylinder and the

Table I. Shewing the expansion of cast brass, both in length and solidity, and also of water, in solidity, by the effect of heat: the former is derived from Mr. SMEATON'S experiments; (Phil.Trans. Vol. XLVIII.) and the latter from some of my own, when I was a resident member of the University of Oxford.

Degrees of Heat.	Expansion of Brass.		Expansion of Water.
	In Length.	In solidity.	In solidity.
o	Millionth Parts.	Millionth Parts.	Millionth Parts.
1	1	3	165
2	2	6	330
3	3	9	495
4	4	12	660
5	5,2	16	825
6	6	19	990
7	7	22	1155
8	8	25	1320
9	9	28	1485
10	10,4	31	1650

Table II. Shewing the Correction for the Wires, or the Diminution of the Weight of the Water-Scales, by Immersion in Water.

By Immersion in Water.	The 4 Wires of the Cube, or Cylinder.	The 3 Wires of the Sphere lose
Inches.	Grains.	Grains.
1	— 0,035	— 0,078
2	— 0,071	— 0,157
3	— 0,106	— 0,235
4	— 0,142	— 0,314
5	— 0,177	— 0,392
6	— 0,212	— 0,471
7	— 0,248	— 0,549
8	— 0,283	— 0,628
9	— 0,319	— 0,706
10	— 0,354	— 0,785
20	— 0,708	— 1,570

N. B. 80 inches in length, of the wires for the scales for the cube and cylinder weigh - - - - - } grs. 6,16
therefore 1 inch will be ,077 grain, and 4 wires of 1 inch - } = 0,308
Also, 91 inches of the wire for the sphere weigh - - - - } = 20,71
and 1 inch = 0,227, and 3 wires of 1 inch - - - } = 0,683
and the specific gravity of the wire is = 8,7

Table III. Shewing the Correction of the Weight of the Sphere in Air, on Account of the Weight, or Heat, of the Atmosphere.

Barometer	Correction.	Therm.	Correction.
Inch. $\frac{1}{10}$.	Grains.	o	Grains.
29,5	0,00	50	0.00
1	— ,12	1	+ 0,10
2	,23	2	0,20
3	,35	3	0,30
4	,47	4	0,40
5	,58	5	0,50
6	,70	6	0,60
7	,82	7	0,70
8	,94	8	0,80
9	1,05	9	0,90
10	1,17	10	1,00

N. B. If the barometer is below $29\frac{1}{2}$ inches, or the thermometer below 50°, use the contrary signs.

Water being taken as heavier than air, as 836 : 1, (see Observations in Savoy, Phil. Trans. for 1777,) the barometer being at 29,27, and thermometer 51°, a sphere of air equal in bulk to the brass sphere, *viz.* = $113\frac{1}{2}$ cubic inches, would weigh, when the barom. was 29,5 inch. and the therm. 50° = 34,57 grains; and 1 cubic inch of such air - = 0,304
This correction will serve for any other body whose bulk is known.

scale in which it hung; these therefore were removed, and the experiment repeated, as follows:

		grains.
Weights as before	No. 12 =	2048
	No. 9 =	256
		200
		30
		10
		4
		1,10
The buoyancy of the air bubbles being removed	= +	3
		1,07
		2553,17
Add the correction for the loss of weight in the wires, as before	= +	0,05
And the more exact weight of the cylinder in water becomes	=	2553,22

with the temperature 60°,5 and the barometer 29,47 inches.

Note, when the cube / the cylinder was weighed in water, its centre was below the surface of the water = 2,5 / 3,7 inches.

that is, the cylinder was the deepest by - - = 1,2

The repetition of this experiment shews how necessary it is to attend to the most trifling circumstances: there were not more than three or four of these particles of air, and those not larger than a small pin's head. Moreover, it may be noted, the distilled water in which these experiments were made, being afterwards examined with my (MARTIN's) hydrometer, in the heat of $60°\frac{1}{2}$, weighed on that scale = 1,0005; so that I see no reason for diffidence in the quality of the water.

(§. 24.) *A Synopsis of the preceding Experiments.*

	Cube.	Therm.	Cylinder.	Therm.	Barom.
	Inches.	°		°	Inches.
Contents (true to $\frac{2}{10000}$) in inches	124,18917	61	74,94826	62	
	grains.				
Weight in air, true to 0,02 grain	32084,82	62	21560,05	62	29,00
Weight in water, true to 0,10 grain	703,03	60,2	2553,22	60,5	29,47
Weight of an equal bulk of water, true to 0,12 grain, or $\frac{1}{190000}$	31381,79		19006,83		
Weight of a cubic inch of water, from these experiments -	252,694		253,600 *		

The diversity in the result of these two experiments is deserving of notice, and must be explained. It may proceed from two causes, which we will now inquire into. But first it may be observed, that the accuracy in measuring the dimensions of these two bodies, as well as the precision in weighing them, has, I think, been such as to put out of all doubt this part of the experiment. From whence then does this difference arise? Either of two causes may be suspected; *viz.* the pressure of the water against the sides of these two bodies altering their volumes, which, it may be presumed, would have a greater effect on the cube, from its figure, than on the cylinder, and in a direction agreeable to this difference; that is, it would diminish the capacity of the cube more than that of the cylinder, and thus make the apparent weight of a cubic inch less in the experiment of the cube. But also we see, that the cylinder was

* The weight of a cubic inch of common or rain water has been reckoned about 253 grains, sometimes = 253,33 grains, at others 253,18. But authors do not seem to have agreed in what they meant by *common* water, *rain* water, *pump* water, *spring* water, and *distilled* water; for occasionally they are all confounded, and made to pass for each other; and sufficient notice seems not to have been taken of the temperature to which these weights were assigned. See MARTIN's *Philosophia Britannica*. LEWIS's Philosophical Commerce of Arts. CHAMBERS's Dictionary, by Dr. REES. *&c. &c.*

weighed at a greater depth, by 1,2 inch, than the cube, below the surface of the water. Now, if it be true that water is compressible,* it will become denser, from its weight, at different depths, and this circumstance would act in the same way with that just mentioned; *viz.* would make the apparent weight of a cubic inch less from the experiment of the cube than the cylinder, which we see is the fact.

(§. 25.) In order to dissipate these doubts, I caused a very accurate hollow brass sphere to be made, of about six inches diameter, and of such thickness of metal, *viz.* 0,13 inch, as to be very little heavier than water, and yet of such strength as, together with its form, to resist any probable change of bulk by the pressure of water.

This sphere, which has already been mentioned, (§. 10.) was examined in the following manner. The six-inch moveable bar *r*, (Tab. V. fig. 3.) of the gauge, was compared with the divided scale of inches, fig. 1. The microscopes being adjusted to exactly six inches, or the interval between 26 inches and 32 inches, and the bar placed under them, the excess above 6 inches was found to be as follows, by the micrometer, *n o*.

	1st trial. inches.		2d. trial, after re-adjustment. inches.	
	6+,0055	thermom. 64°,0	6+,0055	therm. 64°,0
	,0053		,0052	
	,0056		,0055	
	,0054		,0054	
	,0057		,0052	
Mean of the 1st trial	=6 ,00550		6 ,00536	
Mean of the 2d trial	=6 ,00536			
Mean of both, or length of bar	=6 ,00543	in the temperature of 64°.		

* See Mr. CANTON's Experiment in the Phil. Trans. Vol. LII.

(§. 26.) The bar was then placed in the rectangular *gauge* *k l m n*, fig. 2. in the direction *p o;* and the end of the micrometer screw brought to bear against it repeatedly, so as to touch without force, or considerable pressure; and the divisions* cut by the index, on the micrometer plate of the gauge, were as follow:

Trial 1st. Division on the micrometer.		Trial 2d. Division on the micrometer.		Trial 3d. Division on the micrometer.	
65	therm. 62°,0	64	therm. 62°,0	$64\frac{1}{2}$	thermom. 62°,3
63		62		65	
66		65		66	
70		63		63	
66		$62\frac{1}{2}$		$62\frac{1}{2}$	
Mean = 66		63,3 †		64,2	

The mean of these three means is 64,5, with the temperature 62°,1.

(§. 27.) The bar was now removed from the gauge, and the sphere put there in its place; and, by means of the three great circles, each of which was divided into 8 equal parts, nine several diameters of the sphere were taken, as follow:

	Div. of microm.			Div. of microm.			Div. of microm.			Div. of microm.	
diam. A B	40	therm. $62°\frac{1}{2}$	diam. G H	40	therm. 62°,5	diam. C D	41	therm. 62°,4	diam. I K	44	therm. 62°,5
	50			42			49			46	
	47			45			43			47	
	42			42			42			45	
	46			44			$44\frac{1}{2}$			46	
Mean	=45			42,6			43,9			45,6	

* Each thread of this screw is = $\frac{1}{103}$ inch, and each revolution of the screw is divided into 100; so that every division on the micrometer plate is = $\frac{1}{10300}$ inch.

† In all these experiments with the gauge, the figures on the micrometer plate increase as the screw goes *forward*; *viz.* the higher numbers indicate a less interval or diameter.

The above four mean dimensions may be called *equatorial, viz.* - - - - - - $\begin{cases} 45 \\ 42,6 \\ 43,9 \\ 45,6 \end{cases}$

The mean of which is - - - - = 44,3

	Div. of micr.			Div. of micr.			Div. of micr.	
diam. EF	44, 46, 44, 45, 45	therm. 62°,5	diam. 1,2.	42, 45, 45, 40, 41	therm. 62°,6	diam. 3,4.	40, 44, 41, 40, 41	therm. 62°,8
Mean	44,8			42,6			41,1	

These three last dimensions, together with the 1st of the preceding set, may be called *meridional,* being in a circle at right angles to the former, *viz.* - - - - - - $\begin{cases} \text{A B} = 45 \\ \text{E F} = 44,8 \\ 1,2 = 42,6 \\ 3,4 = 41,1 \end{cases}$

The mean of which is - - - - - = 43,4

and differs from the former not quite $\frac{1}{10,000}$ inch.

In another great circle, 90° from the preceding, comprising the diameters already taken, E F and C D, at the intersection of the two former circles, were taken

	Div. of micr.			Div. of micr.	
The diameter $\alpha\,\beta$	41, 44, 44, 41, 40	therm. 63°	diameter $\gamma\,\delta$	40, 41, 42, 40, 42	therm. 63°,1

The diameters

E F, C D } taken as before { 44,8 / 43,9

$\alpha\,\beta$ - - 42,5

$\gamma\,\delta$ - - - 41,0

Mean - - = 43,0, which is that of another great circle

or meridian, at right angles to the former; from whence it will be seen, that not one of the three circles differs from another more than about $\frac{1}{10,000}$ inch.

The preceding 9 mean dimensions of the diameter, collected, are

$$\left.\begin{array}{ll} A\,B & = 45, \\ C\,D & = 43{,}9 \\ G\,H & = 42{,}6 \\ I\,K & = 45{,}6 \\ E\,F & = 44{,}8 \\ 1{,}2 & = 42{,}6 \\ 3{,}4 & = 41{,}1 \\ \alpha\,\beta & = 42{,}5 \\ \gamma\,\delta & = 41{,}0 \end{array}\right\} \begin{array}{l} \text{the mean of which is} = 43{,}7 \\ \text{in the temperature} \quad - \quad 62^\circ{,}6 \end{array}$$

Now the import of the foregoing experiments is this, that when the mean diameter of the sphere is holden between the points of contact of the gauge, near *o* and *p*, the index of the micrometer shews - - - = 43,7 divis.
but, when the bar *r* is placed there, it shews = 64,5

the difference is - - - = 20,8
and by so much is the bar shorter than the diameter of the sphere.

	inches.
These divisions, 20,8, are equal to (§. 26.) -	0,00202
and the length of the bar has already (§. 25.) been found	=6,00543
therefore the true diameter of the sphere becomes	=6,00745

which quantity I think must be true to within $\frac{1}{10,000}$ inch.

(§. 28.) The cube of this diameter, 6,00745 inches × ,5236, as is well known, will give the contents of the sphere in cubic inches, *viz.* = 113,5194 inches, which must be very near the truth: for, if not, let it be supposed that the inaccuracy in the measurement, or the irregularities in the figure of this sphere,

should be such as to amount to $\frac{1}{1000}$ inch, and these so many, without balancing each other, as to produce a spheroidical form, one of whose diameters should exceed the other by $\frac{1}{1000}$ inch; in that case, the error in the assumed solid would not exceed $\frac{1}{3000}$ part of the whole; and this is a position infinitely too extravagant to be admitted, when we recollect, that this diameter has been probably taken to within a tenth part of that error.

(§. 29.) The weight of this sphere, in air and in water, comes next under our consideration; the experiments for which were as follow, made June 12, 1797; the barometer being at 29,74 inches, and the thermometer, in air, at 67°.

Experiment the 1st.

The weight of the sphere in air, the counterpoise, or weight of the scale or cradle, *abcf*, (Tab. VII. fig. 3.) in which the sphere hung, being allowed for*, so that *this* was the net weight -		Troy grains. = 28722,64
The sphere and scale suspended in water, with its centre 5,6 inches below the surface, and the heat 66°	grains. = 303,17	
Deduct the counterpoise, or weight of the scale, in water, with the same heat of 66°, and same depth† below the surface -	= — 253,32	
The difference is the net weight of the sphere in water, of the temperature 66°, which, deducted from its weight in air - -		— 49,85
Leaves the weight of a bulk of water = the sphere, in the temperature 66°, and 5,6 inches below the surface - - - - -		= 28672,79

* The weight of this scale, with its 3 wires, in air, was = 276,10 grains.

† The sphere having been weighed in the same depth of water that the counterpoise to the scale was determined in, no correction for the greater or less immersion of the scale-wires was here necessary; which however will sometimes be the case. See §. 29. and table II. of correction, §. 23.

Experiment the 2d. June 16, 1797.

The barometer being at 30,13 inches, and the thermom. at 68°.

		grains.
Weight of the sphere, together with the scale, in air		29265,91
Deduct the weight of the scale, or counterpoise, in air - - - -	= −	544,03
Remains the total net weight of the sphere in air	=	28721,88
And, to reduce this to the same state of the atmosphere as the preceding observation, *viz.* 29,74 inches of the barometer, add the correction for 0,39 inch (see table, §. 23.)	= +	,46
Also the correction for 1° of the thermometer	= +	,08
And the net weight of the sphere, in an atmosphere of 29,74 inches, and heat of 67°, becomes	=	28722,42

	grains.
Weight of the sphere, with its scale, in water, 3,7 inches below the surface, and the thermometer at 66°,1 - - - -	= 484,70
From thence deduct the weight of the scale in water - -	= 435,09
The net weight of the scale in water becomes - -	= 49,61
To which, add the correction for the wires of the scale being immersed 2,53 inches deeper now, than when its weight in water was determined (see table, §. 23.) -	= 0,20

And the corrected net weight, in water, is - -		49,81
Which, deducted from its weight in air, leaves the weight of a bulk of water = the sphere, in temperature 66°,1 - -	=	28672,61
Correction for 0°,1 of heat* - -	+	,45
And the true corrected weight of a bulk of water equal to the sphere, reduced to the barometer = 29,74, and therm. 66°,0, becomes	=	28673,06

* One degree difference of heat in the water will alter the weight of the sphere in water, or the weight of the bulk of water equal to it, = 4,54 grains; so that, by far the greatest source of error, in these experiments, lies in the difficulty of exactly knowing, and preserving, the temperature of the water.

Experiment the 3d. June 16, 1797.

		grains.
The true net weight of the sphere in air, reduced to a state of the barometer of 29,74 inches, and therm. 67°, as in last experiment	=	28722,42
	grains.	
Weight of the sphere, together with its scale, in water, 6,8 inches below the surface; the thermom. at 66°,1	= 484,20	
Deduct the weight of the scale in water	435,09	
The difference is the net weight of the sphere in water, of the temperature 66°,4 - -	= 49,11	
To which, add the correction for the wires of the scale being immersed 5,5 inches deeper now, than when its weight in water was determined (see table, §. 23.) -	+ ,44	
The corrected net weight, in water, becomes	=	49,55
Which, deducted from its net weight in air, leaves the weight of a bulk of water = the sphere, and 6 inches below the surface, with the heat of 66°,4 - - - - -	=	28672,87
Correction for 0°,4 of heat (see table, §. 23.)	= +	1,81
The true corrected weight of a bulk of water = the sphere, in the heat of 66°,0, and with a pressure of the barometer of 29,74 inches, and 6 inches below the surface -	=	28674,68

(§. 30.) *Results of the Observations of the Sphere collected.*

Correct weight of a bulk of water = sphere, the barom. being at 29,74 inches, therm. 66°,0.	grains.	At a depth below the surface of the water. inches.
By the 1st observation - - -	28672,79	5,6
2d observation - - -	28673,06	3,7
3d observation - - -	28674,68	6,8
Mean of all - - - - -	28673,51	5,37

Which, I think, may fairly be presumed to be within 1 part in 50,000 of the truth.

(§. 31.) Now the contents of this sphere having already (§. 28.) been found to be = 113,519 cubic inches; $\frac{28673,51}{113,519}$ = 252,587 grains, will be the weight of a cubic inch of distilled water, under the circumstances above mentioned, by Mr. TROUGHTON's weights.*

I think it may now be concluded, that the variety in the experiments of the cylinder and the cube, (§. 24.) does not proceed from the different depths † in the water, at which they were made; at least, that the pressure of 3 inches, in perpendicular height of water, does not render that fluid more dense by $\frac{1}{20,000}$ part, which may be reckoned an insensible quantity; but that this variety *did* proceed from a difference in the yielding of the sides of the cube and the cylinder. And lastly, I hope it may be trusted, that the weight of a bulk of water

* But, as will appear hereafter, (§. 41.) these weights are too light, when compared with the standard in the House of Commons, by about 1 in 1523,92; the correction therefore, for this difference, would be = 0,165 grain, to be deducted from - - - - - - 252,587 grains.
— ,165

And the weight of a cubic inch of distilled water, in grains of the parliamentary standard, will be - - - } = 252,422

† By means of an alteration and addition to my apparatus, since the experiment abovementioned was made, I have been able to repeat it at greater depths below the surface of the water, *viz.* when the centre of the sphere was 5 inches, 13 inches, and 21 inches, below, without any appearance of water having a sensible difference of density at different depths. The vessel I used for this purpose was of wood, 32 inches high, and 12 square, containing 16 gallons, with two sides of plate-glass, to admit the light; and the wires by which the sphere was suspended were 45 inches long, and stronger than before, *viz.* 100 inches of the single wire weighed 24,14 grains; and due allowance was made for the different weight of the scale and wires, in air and water, from actual experiment.

= the sphere, has been determined to within $\frac{1}{30,000}$ of the whole, and probably to within half that quantity.

(§. 32.) Having then, through the means of Mr. WHITEHURST's observations, and of his own instrument, ascertained the length of his proposed standard, in the latitude of London, 113 feet above the level of the sea*, under a density of the atmosphere corresponding to 30 inches of the barometer, and 60° of the thermometer, which is full as satisfactory, for all practical purposes, as if it had been done *in vacuo*†, which I conceive to be nearly impossible; and, having determined the weight of any given bulk of water, compared with this common measure; I believe it now only remains, to ascertain the proportion of this common measure and weight, to the commonly received measures and weights of this kingdom.

(§. 33.) It is perfectly true, that if I chose to indulge in fanciful speculation, I might neglect these comparisons, as an unphilosophical condescension to modern convenience, or to ancient practice, and might adopt some more magnificent integer than the *English pound* or *fathom;* such as the *diameter* or *circumference* of the world, *&c. &c.* and, without much skill in the learned languages, and with little difficulty, I might ape the barbarisms of the present day. But in truth, with much inconvenience, I see no possible good in changing the quantities, the divisions, or the names of things of such constant recurrence in common life; I should therefore humbly submit it to the good sense of the people of *these* kingdoms at least, to

* The height, as I have been informed, of the room of Mr. WHITEHURST's observations.

† It is perfectly true, that this supposes the experiment to be made with a pendulum similar to Mr. WHITEHURST's.

preserve, with the measures, the language of their forefathers. I would call a yard a yard, and a pound a pound, without any other alteration than what the precision of our own artists may obtain for us, or what the lapse of ages, or the teeth of time, may have required.

(§. 34.) The difference of the length of the two pendulums, from Mr. WHITEHURST'S observations, appearing to be 59,89358 inches, on Mr. TROUGHTON'S scale; and a cubic inch of distilled water, in a known state of the atmosphere, having been found to weigh 252,587 Troy grains, according to the weights of the same artist, it remains only to determine the proportions of these weights and measures, to those that have been usually, or may be fitly, considered as the standards of this kingdom; and herein a small discrepancy between themselves, in these authoritative standards, will have no influence on the general conclusion I propose to draw; which is, not so much to say what *has* been the standard of Great Britain, as what it *shall* be henceforward, and may be immutably *so;* and which shall differ but a very small quantity, and that an assignable one, from those that have been in use for two or three hundred years past. By these means, no inconvenience would be produced from change of terms, or subdivisions of parts, or from sensible deviation from ancient practice: all that will be done, will be to render that certain and permanent, which has hitherto been fluctuating, or liable to fluctuation. To give effect and energy to these suggestions, is the province of another power.

(§. 35.) The chief standards of longitudinal measure, as far as I can learn, that carry any authority with them, are those preserved in the Exchequer; in the House of Commons; at the

Royal Society; and in the Tower. The first alone, indeed, bear legal authority, and have been in use for more than 200 years; the last is considered as a copy of them, and is not used for sizing generally. The two remaining ones are of modern date: and, although they do not carry with them *at present* any statuteable authority, yet, from the high reputation and acknowledged care of the artists who made them, (the celebrated Mr. GEORGE GRAHAM, and Mr. JOHN BIRD,) are undoubtedly entitled to very great respect; and are probably derived from a mean result of the comparisons of the old and discordant ones in the Exchequer. I shall begin with that of Mr. GRAHAM, which contains also the length of the Tower standard laid down upon it; will proceed then to Mr. BIRD's, and finally conclude with those at the Exchequer.

(§. 36.) May 5, 1797. I went to the apartments of the Royal Society, at Somerset House, and, with the ready assistance of Mr. GILPIN, at the kind instance of Sir JOSEPH BANKS, I made the following observations on Mr. GRAHAM's* brass standard yard, made in 1742. This scale is about 42 inches long, and half an inch wide, containing three parallel lines engraven thereon, on the exterior and ulterior of which are three divisions, expressing feet; with the letter E at the last division, and, by a memorandum preserved with it in the archives of the Society, is said to signify English measure, as taken from the standard in the Tower of London. That with the letter F denoting the length of the half of the French toise; put on here, by the authority and under the inspection of the Royal Academy of Sciences, then

* This rod was not made by Mr. GRAHAM, but, at his instance, procured by him from Mr. JONATHAN SISSON, a celebrated artist of that time. See Phil. Trans. Vol. XLII.

subsisting at Paris, to whom it was sent in 1742, for the purpose of comparing the French and English measures. The middle line, marked EXCH. of the three abovementioned, denotes, as is supposed, the standard yard from the Exchequer.

(§. 37.) This bar of Mr. GRAHAM'S had been previously laid together with my scale divided by Mr. TROUGHTON, for twenty-four hours, to acquire the same temperature; they were also of the same metal, and, by placing it under my microscopes, adjusted to the interval between 10 and 46 inches, I found the interval on the Tower standard exceed

$$\text{mine, by} - \left.\begin{array}{r} \text{Inches.} \\ 0,00127 \\ ,00135 \\ ,00128 \\ \hline \text{Mean} = \quad ,00130 \end{array}\right\} = \text{the total length therefore } 36,00130 \text{ inches, the thermometer at } 60°,8.$$

The interval on the line marked EXCH. was shorter than

$$\text{mine by} \quad - \quad - \left.\begin{array}{r} \text{Inches.} \\ ,0066 \\ ,0066 \\ ,0068 \\ \hline ,0067 \end{array}\right\} = \text{the total length} = 35,9933 \text{ inches, the therm. at } 60°,6.$$

And the Paris half-toise, which had been supposed by the Academy to be = 38,355 English inches, was found, compared

$$\text{with mine, to be} = \left.\begin{array}{r} \text{Inches.} \\ 38,3561 \\ ,3563 \\ ,3559 \end{array}\right\} \text{Mean} = \overset{\text{Inches.}}{38,3561}\text{*}.$$

* Dr. MASKELYNE says, this standard yard of Mr. GRAHAM'S was $\frac{1}{1000}$ inch longer on three feet than Mr. BIRD'S divided scale, which he generally made use of in all his operations of dividing; and, from one made conformably to this of Mr. BIRD'S, Mr. TROUGHTON divided my scale of 60 inches. This remark seems to

	Inches.
The 1st of the preceding observations giving - -	36,0013
The 2d - - - -	35,9933
The mean length of Mr. GRAHAM's standard becomes	35,9973.

agree with my 1st and 3d comparison, but not with the intermediate one. See Phil. Trans. for 1768, p. 324.

As I am now upon the subject of foreign measure, it may not be quite out of place to say a word on the length of the ancient Roman foot, which I am enabled to do with some precision.

Some years ago, when I was in Italy, I had several opportunities of ascertaining the length of this measure, by actual examination of the Roman foot rules, of which I have met with nine, *viz.* two in the Capitol at Rome; one in the Vatican; five in the Museum at Portici, near Naples; and lastly, one in the British Museum, sent from Naples by Sir WILLIAM HAMILTON. They were all of brass, except one half-foot, of ivory, with a joint in the middle, resembling our common box or ivory rules: and, by reference to my journal kept at that time, I find the mean result from all the nine rules, *viz.* by taking both the whole and the parts of each, (for they were divided into 12 inches, and also into 16ths, or digits,) gave, for the length of the old Roman foot, in English inches, correspondent to Mr. BIRD's measure, = 11,6063.

In confirmation also of this conclusion, and agreeably to the idea of Mons. DE LA CONDAMINE, in the "Journal of his Tour to Italy", I took the dimensions of several ancient buildings, *viz.* the interior diameter of the temple of Vesta; the width of the arch of Severus; the door of the Pantheon; and the width of the base of the quadrilateral pyramid of Cestius, which, it is curious to observe, I found exactly 100 old Roman feet, and 125 feet high. This I do not remember to have seen noticed by any former traveller.

The mean result of these experiments gave me -	11,617 English inches.
Ditto, as before, from the rules - - -	11,606 ditto.
The mean of the two modes of determination is -	11,612 ditto.

I may add, that in the Capitol is a stone, of no very ancient date however, let into the wall, on which is engraven the length of several measures, from whence I took the following:

The ancient Roman foot, =	11,635	English inches.
The modern Roman palm, =	8,82	ditto.
The ancient Greek foot, =	12,09	ditto.

(§. 38.) From the information in the report of a committee of the House of Commons, that sat in the year 1758, I learnt that Mr. BIRD'S parliamentary standard had been in the custody of some of its officers, but of whom nobody knew: however, under the authority of the speaker, who was so good as to furnish me with a room in his house, to make the comparisons in, I at last discovered this valuable original in the very safe keeping of ARTHUR BENSON, Esq. Clerk of the Journals and Papers, and which, I believe, had never seen the light for five-and-thirty years before. It is a brass rod or bar, about 39 inches long, and 1 inch square, inclosed in a mahogany frame, inscribed " Standard Geo. 2d. 1758"; at each extremity of it is a gold pin, of about $\frac{1}{10}$ inch in diameter, with a central point, and these points are distant = 36 inches. It bears, however, no divisions; but there was found with it, in another box, a scale divided into 36 inches, with brass cocks at the extremities, for the purpose of sizing or gauging other scales or rules by. Besides these, I found another standard, in size, and in all respects, similar to the last, inscribed 1760, having been made for another committee, that sat in that year; this also was accompanied with a similar divided scale of 36 inches.

These bars being too thick to be conveniently placed under the microscopes of my instrument, the interval of 36 standard inches was laid down on my scale with a beam-compass, two fine points made, and, compared with TROUGHTON'S divisions, was = 36,00023 inches; the thermometer being at 64°. I then examined the other standard, marked " Standard, 1760",

and found it to agree exactly with that of 1758; at least it did not differ from it more than ,0002 inch*.

(§. 39.) I was now to examine the old standards kept in the Exchequer: these Mr. CHARLES ELLIS, Deputy Chamberlain of the Tally Court at the receipt of the Exchequer, was so good as to supply me with; *viz.* the standard yard of the 30th of Eliz. 1588, and also the standard ell of the same date. These are what have been constantly used, and are indeed the only ones now in use, for sizing measures of length†. They are made of brass, about 0,6 inch square, and are very rudely divided indeed, into halves, quarters, eighths, and sixteenths; the lines being two or three hundredths of an inch broad, and not all of them drawn square, or at right angles to the sides of the bar, so that no accuracy could possibly be expected from such measures. However, the middle point of these transverse lines, between the sides of the bar, was taken as the intended original division; and these divisions, such as they were, were transferred, by a dividing knife, to the reverse side of my brass scale made by Mr. TROUGHTON, the thermometer being at 63°; and, at my leisure afterwards, I found as follows.

The ends of these venerable standards having been bruised a little, or rounded, in the course of so many years' usage, I conceived a tangent to be drawn to the most prominent part, which was about the centre or axis of the bar, and this point

* These quantities then being so small, I shall consider them as wholly insensible; and shall say, that Mr. BIRD's parliamentary standards of 3 feet exactly correspond with Mr. TROUGHTON's scale.

† There was also a standard yard of Henry VII. but of very rude workmanship indeed; now quite laid by, and at what time last used, no information remains: but of this more hereafter.

being referred to TROUGHTON's scale, between 6 and 42 inches, the entire yard of 1588, measuring from one extremity to the other, was found to be shorter than this, by —,007 inch: but these comparisons will be better exhibited in a table.

Exchequer standard of 1588.	Difference from Troughton.	Length in Inches.	Difference on 36 inches.	Mean difference on 36 inches.
	Inch.			
Entire yard - -	— ,007	35,993	— ,007	
$\frac{1}{2}$ yard, from 24 to 42 inch.	+ ,063	18,063	+ ,126	Inch.
$\frac{3}{4}$ yard, from 15 to 42 inch.	— ,008	26,992	— ,011	= + 0,015
$\frac{7}{8}$ yard, from 10$\frac{1}{2}$ to 42 in.	+ ,022	31,522	+ ,025	
$\frac{15}{16}$ yard, from 8$\frac{1}{4}$ to 42 in.	— ,055	33,695	— ,059	
Entire ell, from 2 to 47 inch. - - -	— ,036	44,964	— ,029	
$\frac{1}{2}$ ell, from 2 to 24$\frac{1}{2}$ inch.	+ ,032	22,532	+ ,052	
$\frac{3}{4}$, from 2 to 35$\frac{3}{4}$ inch.	+ ,017	33,767	+ ,018	= + 0,016
$\frac{7}{8}$, from 2 to 41,375 inch.	— ,001	39,374	— ,001	*Viz.* the Exchequer measure is by so much the longer, or about 1 in 2322.
$\frac{15}{16}$, from 2 to 44,1875 inch. - -	+ ,051	42,239	+ ,043	

(§. 40.) It appears then, from the above table, that the ancient standards of the realm differ very little from those that have been made by Mr. BIRD, or Mr. TROUGHTON, and consequently, even in a finance view, (if one might look so far forward,) nothing need be apprehended, of loss in the customs, or excise duties, by the adoption of the latter.

(§. 41.) I shall now endeavour to shew the proportion of the *weights* that I have used, compared with the standards that were made by Mr. HARRIS, Assay Master of the Mint, under the orders* of the House of Commons, in the year 1758. They are kept in the same custody with Mr. BIRD's scales of length, and appear to have been made with great care, as a mean result from a great number of comparisons of the old weights in the Exchequer, which have been detailed at length in that report. Mr. HARRIS having been of opinion that the Troy pound was the best integer to adopt, as the standard of weight, I venture to conclude that *this* was the most accurate, and most to be depended upon, of all the various weights and duplicates that he made for the use of this committee; for he made them of 1, 2, 4, 8, 16, lb. and of $\frac{1}{2}$, 1, 2, 3, 6 ounces. It will therefore be sufficient for my purpose, to compare the 1 and 2 pounds Troy, and their duplicates, with the weights of Mr. TROUGHTON.

I did this, June 2d, 1797; the barometer being at 29,72 inches, and thermometer 67°.

	TROUGHTON's weights.		
	lb.	grains.	grains.
The standard weight of 1 Troy pound, or 5760 grains, marked 1758, kept at the House of Commons, in a small box by itself, by Mr. BENSON, weighed -	= 1	3,75 ,74	= 5763,745
A duplicate of the preceding, kept with some other weights, in a box marked B	= 1	3,70 ,67	= 5763,685
The mean weight of the Troy pound, from these two - - - - - -			= 5763,715

* See the report referred to in the note of page 148.

	TROUGHTON'S Weights. grains.	grains.
The two-pounds weight, from the House of Commons, kept in a deal box, marked A - =	10000 1000 400 100 20 7 0,84	= 11527,84
A duplicate of the last mentioned 2 lb. weight, preserved in a deal box, marked B - - - - The thermometer *now* stood at 68°.	10000 1000 400 100 20 7 0,55	= 11527,55

Therefore the mean weight of 2 lb. Troy, from the two last trials, is - - - = 11527,70

And consequently 1 lb. becomes - - - 5763,85

But, from the examination of the two single pound weights, as above, 1 pound is - - 5763,71

Therefore the mean of all is - - - = 5763,78

That is, Mr. TROUGHTON'S weights are too light by $\frac{3,78}{5760,00} = 0,6562$ grain on 1000 grains, or 1 in 1523,92 grains.

(§. 42.) In conclusion, it appears then that the difference of the length of two pendulums, such as Mr. WHITEHURST used, vibrating 42 and 84 times in a minute of mean time, in the latitude of London, at 113 feet above the level of the sea, in the temperature of 60°, and the barometer at 30 inches, is = 59,89358 inches of the parliamentary standard; from whence all the measures of superficies and capacity are deducible.

That, agreeably to the same scale of inches, a cubic inch of pure distilled water, when the barometer is 29,74 inches, and

thermometer at 66°, weighs 252,422 parliamentary grains; from whence all the other weights may be derived.

As a summary of what has been done, I hope it may now be said, that we have attained these three objects;

1st. An invariable, and at all times communicable, measure of Mr. BIRD's scale of length, now preserved in the House of Commons; which is the same, or agrees within an insensible quantity, with the ancient standards of the realm.

2dly. A standard weight of the same character, with reference to Mr. HARRIS's Troy pound.

3dly. Besides the quality of their being invariable, (without detection,) and at all times communicable, these standards will have the additional property of introducing the least possible deviation from ancient practice, or inconvenience in modern use.

(§. 43.) Before I close this Paper, after having said so much on the subject of weights and measures, it may not be improper to add a few words upon a topic that, although not immediately connected, has some affinity to it; I mean the subject of the prices of provisions, and of the necessaries of life, *&c.* at different periods of our history, and, in consequence, the depreciation* of money. Several authors have touched incidentally upon this question, and some few have written professedly upon it; but they do not appear to me to have drawn a distinct conclusion from their own documents. It would carry me infinitely too wide, to give a detail of all the facts I have collected; I shall therefore content myself with a general table of their

* The various changes that have taken place, by authority, in different reigns, either in the weight or alloy of our coins, are allowed for in the subsequent table.

results, deduced from taking a mean rate of the price of each article, at the particular periods, and afterwards combining these means, to obtain a general mean for the depreciation at that period; and lastly, by interpolation, reducing the whole into more regular periods, from the Conquest to the present time: and, however I may appear to descend below the dignity of philosophy, in such œconomical researches, I trust I shall find favour with the historian, at least, and the antiquary.

[*To face p.* 176.

A Table exhibiting the Prices of various Necessaries of Life, together with that of Day Labour, in sterling Money, and also in Decimals, at different Periods, from the Conquest to the present Time, derived from respectable Authorities; with the Depreciation of the Value of Money inferred therefrom. To which is added, the Mean Appreciation of Money, according to a Series of Intervals of 50 Years, for the first 600 Years; and, during the present Century, at shorter Periods, deduced by Interpolation.

	THE PRICES OF VARIOUS ARTICLES AT DIFFERENT TIMES.																				
		MISCELLANEOUS ARTICLES.												Mean Depreciation from these 12 Articles	Beef and Mutton, per lb.	Labour in Husbandry, per Day.	Depreciation of Money, according to the Price of				
		Cattle in Husbandry.					Poultry.														
Year of our Lord	Wheat, per Bushel.	Horse.	Ox.	Cow.	Sheep.	Hog.	Goose.	Hen.	Cock.	Butter, per lb.	Cheese, per lb.	Ale, per Gallon.	Small Beer, per Gallon.				Wheat.	12 miscellaneous Articles.	Meat.	Day Labour.	Mean of all.
	s. d.	£. s. d.	£. s. d.	£. s. d	£. s. d.	£. s. d.	s. d.	s. d.	s. d.	d.	d.	s. d.	d.		d. qr.	s. d.					
1050	0 2¼	1 17 6 * 89	0 7 6 20	0 6 0 37	0 1 3 29	0 2 0 36								42			10	42			26
1150	0 4½	0 12 5	0 4 8¼		0 1 8	0 3 0		0 3								0 2					
1250	1 7¾	1 11 0	1 0 7	0 17 0	0 1 7		1 0	0 3	0 4½												
1350	1 10½	0 18 4 43	1 4 6 66	0 17 2 106	0 2 7 61	0 2 6 45	0 9 75	0 2 24	0 3¾ 31					56		0 3	100	56		75	77
1450	1 5		1 15 8	0 15 6	0 4 11½	0 5 1	0 6¼									0 3¾					
1550	1 10½	2 2 0 100	1 16 7 100	0 16 0 100	0 4 3¾ 100	0 5 6 100	1 0 100	0 8¼ 100	1 0 100	5 100	2 100	0 1½ 100	1 100	100	1 0½	0 4	100	100	100	100	100
1600	4 0½											0 4	2		1 2	0 6					
1625	4 11						2 0		1 6							0 6½					
1650	5 6											0 4	2								
1675	4 6	5 10 0 250	3 6 0 184	2 17 0 345	0 11 0 256	0 14 0 254	3 0 300	1 3 182	1 3 125	4½ 90	2 100	0 8 530	2½ 250	239	1 3½	0 7½	246	239	166	188	210
1700	4 9½											0 10	3								
1720	4 4½											1 0	3		2 2	0 8					
1740	3 8	10 0 0 476	8 0 0 437	7 7 0 884	1 6 0 602	1 15 0 634	3 6 350	1 6 218	1 6 150	9 180	3½ 175	1 0 800	3 300	434	3 0	0 10	197	434	266	250	287
1760	3 9¾	14 0 0 667	8 10 0 465	7 0 0 874	1 7 0 626	1 15 0 634	5 0 500	1 10 266	1 10 183	10 200	5½ 262	1 2 930	3 300	492	4 2	0 11	203	492	400	275	342
1780	4 5½															1 2					
1795	7 10	19 0 0 904	16 8 0 890	16 8 0 2000	1 18 0 882	5 8 0 1960	3 0 300	1 6 218	1 6 150	11½ 230	5 250	1 2½ 969	2¾ 275	752	5 3	1 5¼	426	752	511	436	531

Mean Appreciation by Interpolation.	
A. D.	
1050	26
1100	34
1150	43
1200	51
1250	60
1300	68
1350	77
1400	83
1450	88
1500	94
1550	100
1600	144
1650	188
1675	210
1700	238
1720	257
1740	287
1750	314
1760	342
1770	384
1780	427
1790	496
1795	531
1800 nearly	562

* The small figures denote the price in decimals, whereof those for the year 1550 may be taken for the integer, *viz.* 100.

Besides most of the old chronicles and historians, the following books were consulted, in constructing the above table; *viz.* Bishop FLEETWOOD's *Chronicon Pretiosum*, 1st and 2d edit. *Liber Garderobæ*, in 1299. The Sketch of the Establishments of this Kingdom, *temp.* ED. III. *et seqq.* by J. BREE, 1791. Collection of Ordinances and Regulations of the Royal Household, in divers Reigns, from Edward III. to King William and Queen Mary, Lond. 1790, 4to. The 11th volume of the *Archæologia*. An Enquiry into the Prices of Wheat and other Provisions in England, from the Year 1000 to 1765, by Mr. COMBRUNE, fol. Lond. by T. LONGMAN, 1768. Dr. SMITH's Wealth of Nations. Sir JAMES STEUART's Political Œconomy; and Dr. HENRY's History.

APPENDIX.

(§. 44.) Since the writing of the preceding Memoir, I have had an opportunity of examining three other scales, divided into inches, or equal parts, of considerable authority in this country, having been executed by the late Mr. J. BIRD. I have also compared the old standard in the Exchequer, of the time of Hen. VII. and which is considered to be the most ancient authority of this sort now subsisting: these observations, I flatter myself, the Royal Society will be desirous of possessing.

(§. 45.) The first of the abovementioned scales belonged to the late General ROY, and was purchased by him at Mr. SHORT's sale, the celebrated optician; it was used by him in his operations of measuring a base line on Hounslow Heath. (See Phil. Trans. Vol. LXXV.) It was originally the property of Mr. G. GRAHAM, has the name of JONATHAN SISSON engraven upon it, but is known to have been divided by Mr. BIRD, who then worked with old Mr. SISSON. It is 42 inches long, divided into tenths, with a vernier of 100 at one end, and of 50 at the other, giving the subdivisions of 500ths, and 1000ths, of an inch.

(§. 46.) The second is in the possession of ALEXANDER AUBERT, Esq. and formerly belonged to Mr. HARRIS, of the Tower; contains 60 inches, divided into 10ths, with a vernier, like that of the preceding. It is one inch broad, and 0,2 thick.

(§. 47.) The third was presented by ALEXANDER AUBERT, Esq. and the late Admiral CAMPBELL, Mr. BIRD's executors, to the Royal Society, in whose custody it now remains. It consists of a brass rod, 92,4 inches long, 0,57 inch broad, and

0,3 inch thick; bearing a scale of 90 inches, or equal parts, each subdivided into 10, with a vernier at the commencement, being a scale of 100 divisions to 101 tenths. *This* has been called Mr. BIRD'S own scale, *viz.* made for his own use; and was the instrument with which he is said to have laid off the divisions of his 8-feet mural quadrants. It is probable that Mr. BIRD made many more of these scales, now in the hands of private persons, (one of which, indeed, I saw at the President DE SARON'S, many years ago, at Paris,) but those have not come to my knowledge.

(§. 48.) In comparing General ROY'S (BIRD'S) scale with Mr. TROUGHTON'S, I found 42 inches of the former were = 42,00010 inches on TROUGHTON'S; (the thermometer 51°,7;) 36 inches were consequently = 36,00008.

		Inches.
And 12 inches on the 1st foot were equal to the 12 inches from 12 to 24 on TROUGHTON'S scale - -	— ,0003 =	11,9997
The 2d foot - - -	+ ,0006	12,0006
The 3d foot - - - -	— ,0004	11,9996
The last foot - - -	+ ,0006	12,0006
The mean foot, therefore, in General ROY'S scale, taken from four different feet, compared with TROUGHTON'S, between the 12th and 24th inch, is as 12 to - - - -		12,00012
That is, General ROY'S scale is longest on 1 foot by so much, and longer on 3 feet by - -		,00036
And the greatest probable error from the inequality in the divisions is about - - - -		,0005
And the mean probable error about - -		,0003

(§. 49.) Mr. AUBERT'S scale, compared with Mr. TROUGHTON'S, was as follows: 58 inches were equal to 57,9982 inches

on TROUGHTON's; (thermometer at 51°,0:) *viz.* Mr. BIRD's measure was shortest ,0018; or, shortest on 36 inches = ,0012,

	Inches.	
And 12 inches, or 1st foot, on Mr. AUBERT's - -	= 11,9999	on Mr. TROUGHTON's scale, from 6 inches to 18 inches; the thermometer being at 50°,0.
2d foot,	= 12,0005	
3d foot,	11,9996	
4th foot,	12,0019	
5th foot,	12,0006	
Therefore the mean foot is -	12,0005	

The greatest error in this scale appears to be about = ,0012
And the mean probable error - - = ,0006.

(§. 50.) The Royal Society's scale, compared, was as follows: 58 inches on Mr. BIRD's were equal to 57,99912 inches on Mr. TROUGHTON's: (thermometer 50°,5)

viz. Mr. BIRD's measure was shortest	,00088
Or shorter on 36 inches - -	,00054
32 inches on the same were equal to -	31,99967
viz. Mr. BIRD's was shortest by -	,00033
Or, on 36 inches, by - -	,00037
The mean of these two comparisons is	,00045

And, by so much, is Mr. BIRD's scale shorter, in three feet, than TROUGHTON's.

	Inches.	
And 12 inches, or 1st foot, of the Royal Society's scale, is -	= 12,00013	on TROUGHTON's scale; the thermometer at 51°.
2d foot of ditto	= 11,99957	
3d foot of ditto	= 12,00027	
4th foot of ditto	= 11,99990	
5th foot of ditto	= 12,00063	
6th foot of ditto	= 11,99823	
7th foot of ditto	= 12,00000	
The mean of these seven feet is	= 11,99982	

And the greatest error in these divisions = ,0008
And the mean probable error - = ,0004.

(§. 51.) Lest, however, it should be suspected, that Mr. TROUGHTON'S scale, with which I have made these comparisons, is not sufficiently correct for this apparent preference, I will now give the result of my examination of that scale, from one end to the other. I set the microscopes to an interval of nearly 6 inches, correctly speaking, it was 6,00013 inches, taken from a mean of the whole scale; and, comparing this interval successively, I found as follows.

	Inches.		Inches.		Inches.	Error, or difference from the mean.
viz. from	0	to	6	-	= 6,00025* - -	+ ,00012
	6	to	12	-	= 6,00013 - -	,00000
	12	to	18	-	= 6,00020 - -	+ ,00007
	18	to	24	-	= 6,00000 -	− ,00013
	24	to	30	-	= 6,00007 - -	− ,00006
	30	to	36	-	= 6,00033 -	+ ,00020
	36	to	42	-	= 5,99980 - -	− ,00033
	42	to	48	-	= 6,00020 -	+ ,00007
	48	to	54	-	= 6,00010 - -	− ,00003
	54	to	60	-	= 6,00023 -	+ ,00010

Mean of all - - = 6,00013.

From whence it appears, that the greatest probable error, without a palpable mistake, in Mr. TROUGHTON'S divisions, is = ,00033 inch; against which, the chance is 9 to 1; and

* It is not pretended, that in this and the foregoing observations, the quantity of any interval can be determined to the precision of the one hundred thousandth part of an inch; but it *is* presumed, that with the assistance of the microscopes, the ten thousandth part of an inch becomes visible; and, as a mean is taken from 3 or 4 times reading off the micrometer at each trial, it has been deemed not unreasonable to set down the quantities to five places of decimals.

the mean probable error = ,00016; and that it is 4 to 1 the error doth not exceed $\frac{2}{10,000}$ inch.

This accuracy is about three times as great as that of Mr. Bird's scales, and about equal to that of the divisions of my equatorial instrument, made by Mr. Ramsden, in 1791. See Phil. Trans. for 1793.

(§. 52.) I now proceed to the examination of the standard rod of Henry VII. which is an octangular brass bar, of about $\frac{1}{2}$ an inch in diameter, with one of the sides rudely divided, into halves, thirds, quarters, eighths, and sixteenths; and the first foot into inches. Each end is sealed with a crowned old English H, , and from hence is concluded to be of the time of King Henry VII. *viz.* about 1490, but is now become wholly obsolete, since the introduction of the standard of Queen Elizabeth; but such as it is, I have thought proper to examine it, and find as follows:

	Inches.	Difference.	Error, or difference on 3 feet.
On this rod, $\frac{1}{3}$, or the 1st foot, is equal to	11,973 on Troughton's.		
the 2d foot is -	11,948		
the 3d foot is -	12,047		
The mean foot is -	11,989	— ,011	— ,033
$\frac{1}{2}$ yard, or 18 inches -	= 17,946	— ,054	— ,108
$\frac{2}{3}$ yard, or 24 inches -	= 23,921	— ,079	— ,118
$\frac{3}{4}$ yard, or 27 inches - -	= 26,937	— ,063	— ,084
$\frac{7}{8}$ yard, or $31\frac{1}{2}$ inches -	= 31,443	— ,057	— ,065
$\frac{15}{16}$ yard, or $33\frac{3}{4}$ inches -	= 33,665	— ,085	— ,091
Entire yard, or 36 inches -	= 35,966	— ,034	— ,034
And the mean yard - -	= 35,924	Mean	— ,076

And, by so much, Mr. Troughton's measure is longest.

And the probable error, in the divisions of this old standard, is about $\frac{3}{100}$ inch.

(§. 53.) It may now be desirable to see the comparative lengths of these various standards and scales, reduced to one and the same measure, *viz.* Mr. TROUGHTON'S.

	Inches on Troughton's.	Difference.	Probable error in division.
36 inches, on a mean, of Hen. VII. standard of 1490, are equal to - -	35,924	—,076	,03
——— of standard yard of Eliz. of 1588	36,015	+,015	,04
——— of standard ell of ditto, of 1588	36,016	+,016	,04
———*of yard-bed of Guildhall, about 1660 - - -	36,032	+,032	
———*of ell-bed of ditto, about 1660	36,014	+,014	
———*of standard of clock-makers' company, 1671 - -	35,972	—,028	
———*of the Tower standard, by Mr. ROWLEY, about 1720 -	36,004	+,004	
——— of GRAHAM'S standard, by SISSON, of 1742, *viz.* line E -	=36,0013	+,0013	
——— of ditto, ditto, *viz.* line EXCH.	=35,9933	—,0067	
——— of Gen. ROY'S (BIRD'S) scale - (all made, probably, between the years 1745 and 1760.)	=36,00036	+,00036	,0003
——— of Mr. AUBERT'S, ditto, ditto	=35,99880	—,00120	,0006
——— of Royal Society's ditto, ditto	=35,99955	—,00045	,0004
——— of Mr. BIRD'S parliamentary standard, of 1758 -	=36,00023	+,00023	
——— of Mr. TROUGHTON'S scale, in 1796 - - -	=36,00000	,00000	,0001.

From hence it appears, that the mean length of the standard yard, taken from the seven first instances in this table, agrees with the quantity assumed by Mr. BIRD, or Mr. TROUGHTON, to within $\frac{3}{1000}$ inch, but that the latter is the longest.

* These four quantities are taken from Mr. GRAHAM'S account, in the Phil. Trans. Vol. XLII.

Fig. 2.

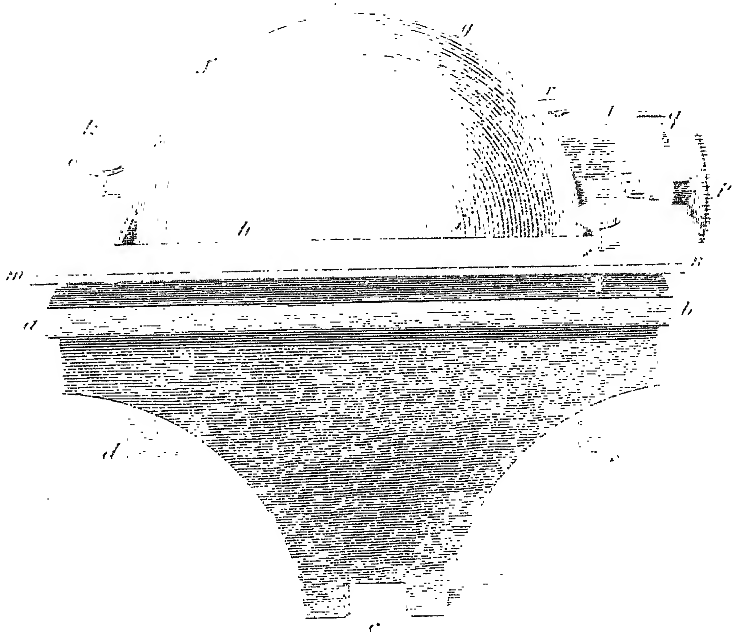
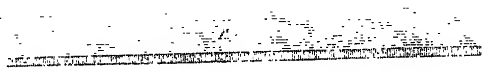


Fig. 3.



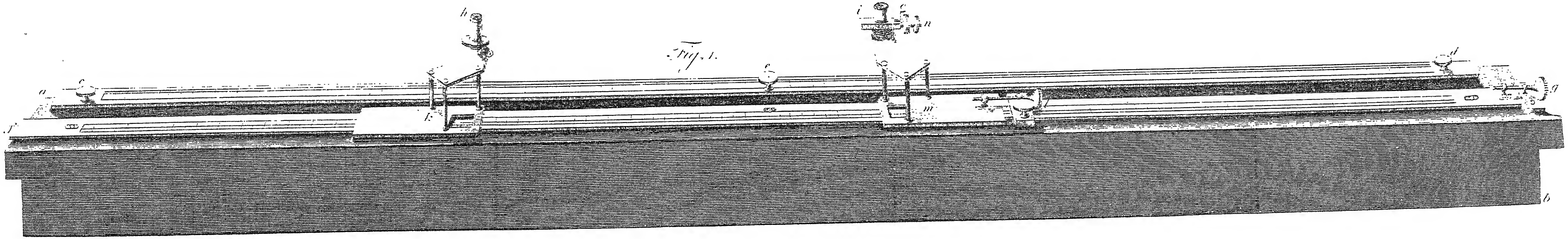


Fig. 1.

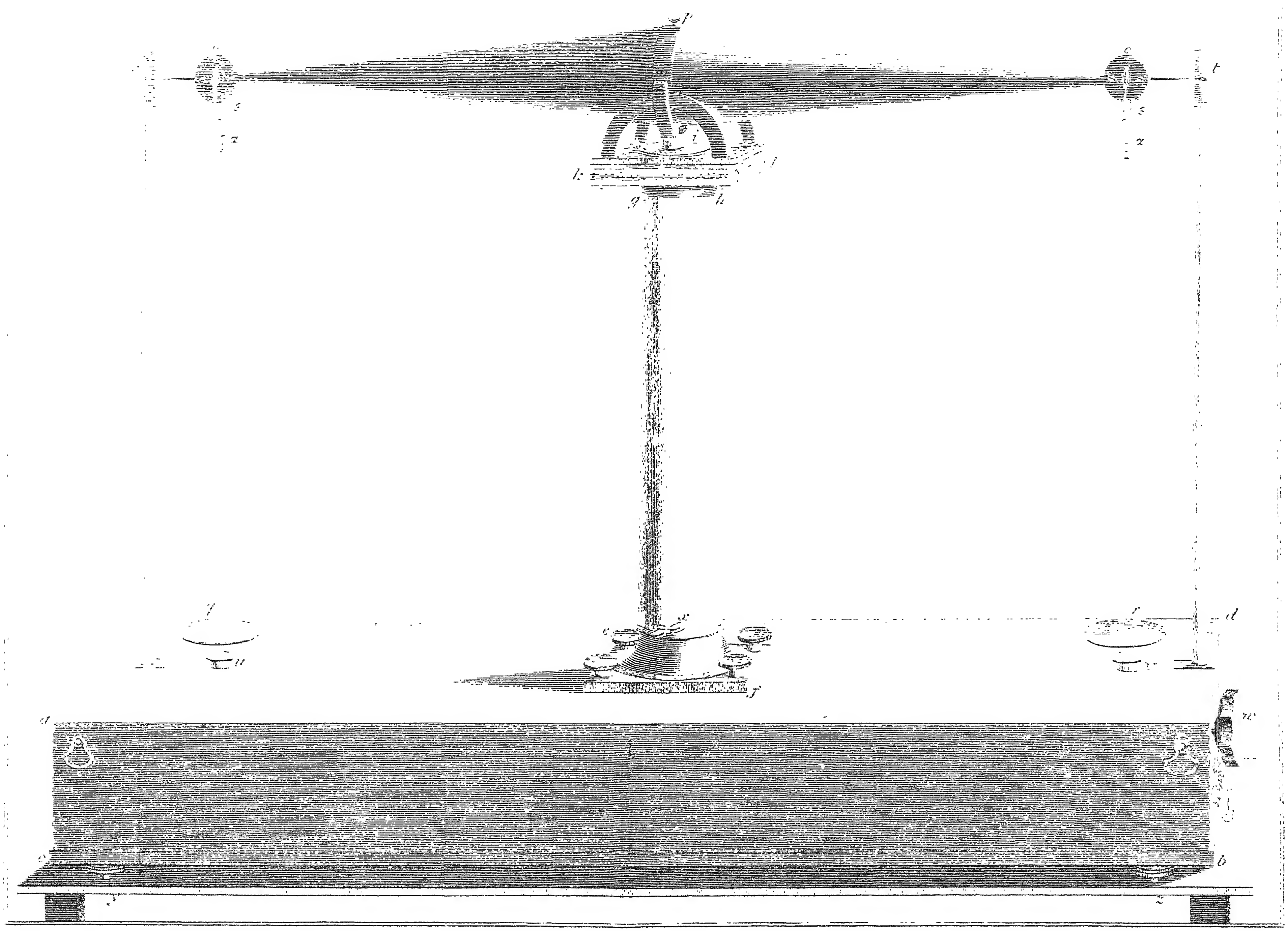

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Fig. 1.

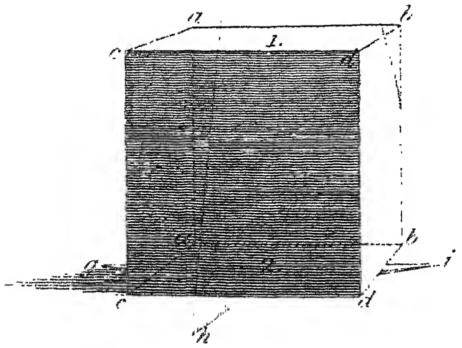


Fig. 3.

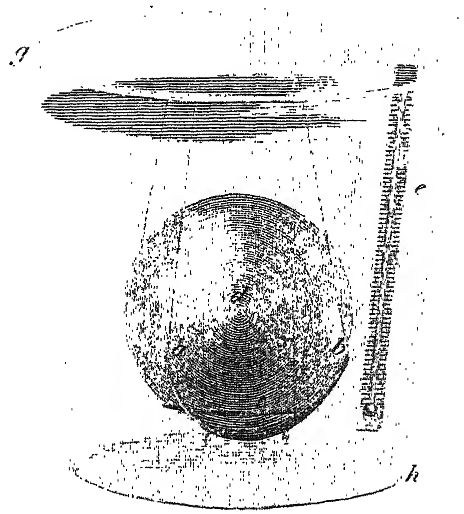
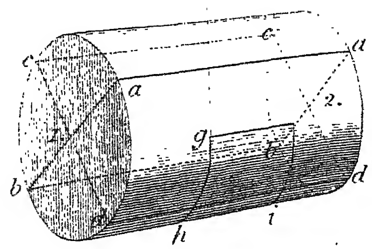


Fig. 2.



IX. *A new Method of computing the Value of a slowly converging Series, of which all the Terms are affirmative. By the Rev.* John Hellins, *F. R. S. and Vicar of* Potter's-Pury, *in* Northamptonshire. *In a Letter to the Rev. Dr.* Maskelyne, *F. R. S. and Astronomer Royal.*

Read March 8, 1798.

REV. SIR, Potter's-Pury, February 8, 1798.

THAT several of the most curious and difficult problems in physical astronomy have hitherto been solved only by means of slowly converging series, is a truth which you are well acquainted with, and which may be seen in the works of the late learned EULER, and others, on that subject. Of this kind of series is the following, *viz.* $ax + bx^2 + cx^3 + dx^4 +$ &c. *ad infinitum*, when all the terms are affirmative, and a, b, c, &c. differ but little from each other, and x is but little less than 1; to obtain the value of which, to seven places of figures, by computing the terms as they stand, and adding them together, is a very laborious and tiresome operation; and therefore some easier method of obtaining it is very desirable. About five years ago, the consideration of this matter was recommended to me by Mr. Baron MASERES, (who not only employs his own leisure in explaining and improving the higher parts of the mathematics, but also encourages others who make the same laudable use of their leisure,) to whom I then communicated the method of computation explained in the paper inclosed in

this letter. As this method is general, for all slowly converging series of the form abovementioned, (which is generally allowed to be the most difficult,) I am induced to present it to you, requesting that, if it meets with your approbation, you will communicate it to the Royal Society.

I am,

Rev. Sir, *&c.*

JOHN HELLINS.

P. S. I need not observe to you, that it is not requisite to the summation of the series mentioned in this letter, that b should be less than a, c less than b, d less than c, *&c.* but only that the first, second, third, *&c.* differences of these coefficients should be a series of decreasing quantities: for, you well know, there are series of that form, which arise in physical astronomy, of which the coefficients are actually a diverging series, and yet the sum of the whole is a finite quantity. And the same thing is evident, from the bare inspection of the theorem which I shall presently use.

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1. The computing of the value of the series $ax + bx^2 + cx^3 + dx^4 +$ *&c. ad infinitum*, in which all the terms are affirmative, and the differences of the coefficients a, b, c, *&c.* are but small, though decreasing, quantities, and x is but little less than 1, is (as has been before observed) a laborious operation, and has engaged the attention of some eminent mathematicians, both at home and abroad, whose ingenious devices on the occasion entitle them to esteem. Of the several methods of

obtaining the value of this series, which have occurred to me, the easiest is that which I am now to describe, by which the business is reduced to the summation of two, three, or more series of this form, *viz.* $ax - bx^2 + cx^3 - dx^4$, &c. and one series of this form, *viz.* $px^n + qx^{2n} + rx^{3n} +$ &c. where n is $= 4$, 8, 16, 32, or some higher power of 2. The investigation of this method is as follows.

2. The series $ax + bx^2 + cx^3 + dx^4 + ex^5 + fx^6 +$ &c. is evidently equal to the sum of these two series, *viz.*

$$ax - bx^2 + cx^3 - dx^4 + ex^5 - fx^6, \text{ \&c.}$$
$$* + 2bx^2 * + 2dx^4 * + 2fx^6, \text{ \&c.}$$

of which, the value of the former is easily attainable, by the method so clearly explained, and fully illustrated, by Mr. Baron MASERES, in the Philosophical Transactions for the year 1777; and the latter, although it be of the same form with the series first proposed, yet has a great advantage over it, since it converges twice as fast. Upon this principle, then, we may proceed to resolve the series $2bx^2 + 2dx^4 + 2fx^6 + 2hx^8 + 2kx^{10} + 2mx^{12} +$ &c. into the two following:

$$2bx^2 - 2dx^4 + 2fx^6 - 2hx^8 + 2kx^{10} - 2mx^{12}, \text{ \&c.}$$
$$* + 4dx^4 * + 4hx^8 * + 4mx^{12}, \text{ \&c.}$$

where, again, the value of the one may easily be computed; and the other, although it be of the same form with the series at first proposed, yet converges four times as fast. And, in this manner we may go on, till we obtain a series of the same form with the series at first proposed, which shall converge 8, 16, 32, 64, &c. times as fast, and consequently a few terms of it will be all that are requisite.

An example, to illustrate this method, may be proper, which therefore is here subjoined.

3. Let it be proposed to find the value of the series $x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} +$ *&c. ad infinitum*, when $x = \frac{9}{10}$.

4. In order to obtain the sum of this series, with the less work, it will be requisite to compute a few of the initial terms, as they stand. For, if we begin the operation with computing the value of $x - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4}$, *&c.* by the differential series before mentioned*, the values of D′, D″, D‴, *&c.* will be $\frac{1}{2}$, $\frac{1.2}{2.3}$, $\frac{1.2.3}{2.3.4}$, *&c.* respectively, *i. e.* $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, *&c.* which is a series decreasing so very slowly, that the only advantage obtained by this transformation of the series is in the convergency of the powers of $\frac{x}{1+x}$ instead of the powers of x, which indeed is very great; for, x being $= \frac{9}{10}$, $\frac{x}{1+x}$ is $= \frac{9}{19}$; so that the new series $\frac{9}{19} + \frac{1}{2} \cdot \overline{\frac{9}{19}}|^2 + \frac{1}{3} \cdot \overline{\frac{9}{19}}|^3 + \frac{1}{4} \cdot \overline{\frac{9}{19}}|^4 +$ *&c.* although $= \frac{9}{10} - \frac{1}{2} \cdot \overline{\frac{9}{10}}|^2 + \frac{1}{3} \cdot \overline{\frac{9}{10}}|^3 - \frac{1}{4} \cdot \overline{\frac{9}{10}}|^4$, *&c.* yet converges more than seven times as swiftly†. But, if we begin the work by computing the first eight terms of the series, as they stand, and then compute the value of $\frac{x}{9} - \frac{x^2}{10} + \frac{x^3}{11} - \frac{x^4}{12} +$, *&c. ad infinitum*, by the same theorem, the values of D′, D″, D‴, *&c.* will be $\frac{1}{9.10}$, $\frac{2}{9.10.11}$, $\frac{2.3}{9.10.11.12}$, *&c.* which is a series decreasing, for a great number

* The theorem best adapted to this business is the following; *viz.* $ax - bx^2 + cx^3 - dx^4$, *&c.* $= \frac{ax}{1+x} + \frac{D' x^2}{(1+x)^2} + \frac{D'' x^3}{(1+x)^3} + \frac{D''' x^4}{(1+x)^4} +$ *&c.* D′ being $= a - b$, $D'' = a - 2b + c$, $D''' = a - 3b + 3c - d$, *&c.*

See *Scriptores Logarithmici*, Vol. III. p. 290, where b, c, d, *&c.* denote the same quantities that a, b, c, *&c.* do here.

† $\frac{9}{19}$ is $= 0.4736842$, and $\overline{\frac{9}{10}}|^7$ is $= 0.4782969$.

of its terms, much more swiftly than the powers of $\frac{9}{10}$, and, in the first seven terms, much more swiftly than the powers of $\frac{9}{19}$. The value of the series $\frac{1}{9}\cdot\frac{9}{10}-\frac{1}{10}\cdot\overline{\frac{9}{10}}|^2+\frac{1}{11}\cdot\overline{\frac{9}{10}}|^3-\frac{1}{12}\cdot\overline{\frac{9}{10}}|^4$, &c. is therefore = the series $\frac{1}{9}\cdot\frac{9}{19}+\frac{1}{9.10}\cdot\overline{\frac{9}{19}}|^2+\frac{2}{9.10.11}\cdot\overline{\frac{9}{19}}|^3+\frac{2.3}{9.10.11.12}\cdot\overline{\frac{9}{19}}|^4+$ &c. the first seven terms of which converge above fourteen times as swiftly as the other; or, in other words, the first seven terms of it will give a result much nearer the truth than a hundred terms of the other. And if, instead of the first eight terms of the proposed series, the first twenty-four terms were computed, as they stand, and then the value of the series $\frac{x}{25}-\frac{x^2}{26}+\frac{x^3}{27}-\frac{x^4}{28}$, &c. by its equivalent, $\frac{1}{25}\,\frac{x}{1+x}+\frac{1}{25.26}\cdot\frac{x^2}{(1+x)^2}+\frac{2}{25.26.27}\cdot\frac{x^3}{(1+x)^3}+\frac{2.3}{25.26.27.28}\cdot\frac{x^4}{(1+x)^4}+$ &c. the rapid decrease of the coefficients $\frac{1}{25}$, $\frac{1}{25.26}$, $\frac{2}{25.26.27}$, &c. compounded with the decrease of the powers of $\frac{x}{1+x}$, (in the present case = the powers of $\frac{9}{19}$,) produces such a very swiftly converging series, that eight terms of it will give the result true to eleven places of decimals.

It may be further remarked, for the sake of my less experienced readers, (for whose information this article is chiefly intended,) that the second term of the last series is produced by multiplying the first by $\frac{1}{26}\cdot\frac{x}{1+x}$; the third, by multiplying the second by $\frac{2}{27}\cdot\frac{x}{1+x}$; the fourth, by multiplying the third by $\frac{3}{28}\cdot\frac{x}{1+x}$; and so on. If, therefore, the first term be called P, the second Q, the third R, the fourth S, &c. we shall have,

$$P = \frac{1}{25} \cdot \frac{x}{1+x},$$
$$Q = \frac{P}{26} \cdot \frac{x}{1+x},$$
$$R = \frac{2Q}{27} \cdot \frac{x}{1+x},$$
$$S = \frac{3R}{28} \cdot \frac{x}{1+x}; \ \&c.$$

which form is well adapted to arithmetical calculation. And, that a similar form for that purpose may always be obtained, when the coefficients of the proposed series are the reciprocals of any arithmetical progression, has been shewn by Mr. SIMPSON, in his Mathematical Dissertations, p. 64.

Having premised these observations, I now proceed to the arithmetical work; in which I shall use, and explain, such other devices as have occurred to me for facilitating it.

5. Since the work of computing the value of the proposed series is so much shortened, by first finding the sum of a moderate number of initial terms, and then resolving the remaining terms into several series, in the manner described in Art. 2, I will begin with computing the first twenty-four terms of it; and, to facilitate this part of the work, I will separate these terms into three parts; *viz.*

$$x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} + \frac{x^7}{7} + \frac{x^8}{8},$$
$$+ \frac{x^9}{9} + \frac{x^{10}}{10} + \frac{x^{11}}{11} + \frac{x^{12}}{12} + \frac{x^{13}}{13} + \frac{x^{14}}{14} + \frac{x^{15}}{15} + \frac{x^{16}}{16},$$
$$+ \frac{x^{17}}{17} + \frac{x^{18}}{18} + \frac{x^{19}}{19} + \frac{x^{20}}{20} + \frac{x^{21}}{21} + \frac{x^{22}}{22} + \frac{x^{23}}{23} + \frac{x^{24}}{24},$$

which are evidently equal to

$$x + \frac{x^2}{2} + \frac{x^3}{3} + \frac{x^4}{4} + \frac{x^5}{5} + \frac{x^6}{6} + \frac{x^7}{7} + \frac{x^8}{8},$$
$$x^8\left(\frac{x}{9} + \frac{x^2}{10} + \frac{x^3}{11} + \frac{x^4}{12} + \frac{x^5}{13} + \frac{x^6}{14} + \frac{x^7}{15} + \frac{x^8}{16}\right),$$
$$x^{16}\left(\frac{x}{17} + \frac{x^2}{18} + \frac{x^3}{19} + \frac{x^4}{20} + \frac{x^5}{21} + \frac{x^6}{22} + \frac{x^7}{23} + \frac{x^8}{24}\right);$$

which three parts, for the sake of reference, may be called A, Bx^8, and Cx^{16}. It is evident also, that the numerical values of the first eight powers of x are all that are required for finding the sum of these twenty-four terms of the series proposed: for $Bx^8 + Cx^{16}$ is $= x^8 (B + Cx^8)$; and this product $+ A =$ the sum required*.

The numerical values of the first eight powers of x are these:

$$x = 0{\cdot}9,$$
$$x^2 = 0{\cdot}81,$$
$$x^3 = 0{\cdot}729,$$
$$x^4 = 0{\cdot}6561,$$
$$x^5 = 0{\cdot}59049,$$
$$x^6 = 0{\cdot}531441,$$
$$x^7 = 0{\cdot}4782969,$$
$$x^8 = 0{\cdot}43046721;$$

from which values of the powers of x, the values of A, B, and C, continued to twelve places of decimals, are easily obtained, and are as follow.

* The same thing might have been obtained from the first four powers of x, or from the first six powers of x; as might likewise the sum of 32, 36, 40, or any other number of terms that is a multiple of 4 or 6. But the number 8 is here chosen, for the sake of a subsequent use that will be made of the value of x^8.

$x = 0{\cdot}9$

$\frac{x^2}{2} = 0{\cdot}405$

$\frac{x^3}{3} = 0{\cdot}243$

$\frac{x^4}{4} = 0{\cdot}16402,5$

$\frac{x^5}{5} = 0{\cdot}11809,8$

$\frac{x^6}{6} = 0{\cdot}08857,35$

$\frac{x^7}{7} = 0{\cdot}06832,81285,71$

$\frac{x^8}{8} = 0{\cdot}05380,84012,50$

The sum is $2{\cdot}04083,30298,21 = A$.

$\frac{x}{9} = 0{\cdot}1$

$\frac{x^2}{10} = 0{\cdot}081$

$\frac{x^3}{11} = 0{\cdot}06627,27272,73$

$\frac{x^4}{12} = 0{\cdot}05467,50000,00$

$\frac{x^5}{13} = 0{\cdot}04542,23076,92$

$\frac{x^6}{14} = 0{\cdot}03796,00714,29$

$\frac{x^7}{15} = 0{\cdot}03188,64600,00$

$\frac{x^8}{16} = 0{\cdot}02690,42006,25$

The sum is $0{\cdot}44412,07670,19 = B$.

$$\frac{x}{17} = 0{\cdot}05294{,}11764{,}71$$
$$\frac{x^2}{18} = 0{\cdot}04500{,}00000{,}00$$
$$\frac{x^3}{19} = 0{\cdot}03836{,}84210{,}53$$
$$\frac{x^4}{20} = 0{\cdot}03280{,}50000{,}00$$
$$\frac{x^5}{21} = 0{\cdot}02811{,}85714{,}29$$
$$\frac{x^6}{22} = 0{\cdot}02415{,}64090{,}91$$
$$\frac{x^7}{23} = 0{\cdot}02079{,}55173{,}91$$
$$\frac{x^8}{24} = 0{\cdot}01793{,}61337{,}50$$

The sum is $0{\cdot}26012{,}12291{,}85 = C$.

We may now quickly find the sum of the first twenty-four terms of the series proposed, by two multiplications by x^8, and two additions, as was pointed out in the former part of this article; but, since the labour of these two separate multiplications may be saved, I shall now proceed to compute the value of the remaining terms of the proposed series.

6. The remaining terms are $\frac{x^{25}}{25} + \frac{x^{26}}{26} + \frac{x^{27}}{27} + \frac{x^{28}}{28} + \frac{x^{29}}{29} + \frac{x^{30}}{30} +$ *&c. ad infinitum*, which, being disposed according to the form mentioned in Art. 2, are evidently equal to $x^{24} \times$ these

$$\text{two series} \left\{ \begin{array}{l} \frac{x}{25} - \frac{x^2}{26} + \frac{x^3}{27} - \frac{x^4}{28} + \frac{x^5}{29} - \frac{x^6}{30}, \text{ \&c.} \\ \quad * \;\; + \frac{2x^2}{26} \quad\;\; * \;\; + \frac{2x^4}{28} \quad\;\; * \;\; + \frac{2x^6}{30} +, \text{ \&c.}; \end{array} \right.$$

the value of the first of which is very easily attainable, in the manner shewn above, in Art. 4. The arithmetical work will stand thus:

$$P = \frac{1}{25} \times \frac{9}{19} = 0{\cdot}01894,73684,2$$

$$Q = \frac{P}{26} \times \frac{9}{19} = 0{\cdot}00034,51949,7$$

$$R = \frac{2Q}{27} \times \frac{9}{19} = 0{\cdot}00001,21121,0$$

$$S = \frac{3R}{28} \times \frac{9}{19} = 0{\cdot}00000,06147,1$$

$$T = \frac{4S}{29} \times \frac{9}{19} = 0{\cdot}00000,00401,6$$

$$U = \frac{5T}{30} \times \frac{9}{19} = 0{\cdot}00000,00031,7$$

$$V = \frac{6U}{31} \times \frac{9}{19} = 0{\cdot}00000,00002,9$$

$$W = \frac{7V}{32} \times \frac{9}{19} = 0{\cdot}00000,00000,3$$

The sum of these terms is 0·01930,53338,5, which, for the sake of reference, call α. Then we have $\alpha x^{24} = x^{24} \times : \frac{x}{25} - \frac{x^2}{26} + \frac{x^3}{27} - \frac{x^4}{28} + \frac{x^5}{29} - \frac{x^6}{30}$, *&c.*

7. The part of the proposed series which now remains to be computed, is $x^{24} \times : \frac{2x^2}{26} + \frac{2x^4}{28} + \frac{2x^6}{30} + \frac{2x^8}{32} + \frac{2x^{10}}{34} + \frac{2x^{12}}{36} +$ *&c. ad infinitum*, or $x^{24} \times : \frac{x^2}{13} + \frac{x^4}{14} + \frac{x^6}{15} + \frac{x^8}{16} + \frac{x^{10}}{17} + \frac{x^{12}}{18}$ + *&c. ad infinitum*, which may also be divided into two series, in the manner above shewn; but, to obtain a swifter convergency in the next, as well as the succeeding applications of the differential series, it will be convenient first to compute the value of four terms at the beginning of this series; which we may quickly do, since all the powers of x requisite in that part of the calculation are ready at hand, being set down in Art. 5. The work will stand thus:

$$\frac{x^2}{13} = 0{\cdot}06230{,}76923{,}1$$
$$\frac{x^4}{14} = 0{\cdot}04686{,}42857{,}1$$
$$\frac{x^6}{15} = 0{\cdot}03542{,}94000{,}0$$
$$\frac{x^8}{16} = 0{\cdot}02690{,}42006{,}3:$$

And the sum is $0{\cdot}17150{,}55786{,}5$, which call D. Then will $Dx^{24} = x^{24} \times \left(\frac{x^2}{13} + \frac{x^4}{14} + \frac{x^6}{15} + \frac{x^8}{16}\right)$.

8. It now remains to find the value of $x^{24} \times : \frac{x^{10}}{17} + \frac{x^{12}}{18} + \frac{x^{14}}{19} + \frac{x^{16}}{20} + \frac{x^{18}}{21} + \frac{x^{20}}{22} +$ &c. *ad infinitum*, or of its equal, $x^{32} \times$ the sum of these two series,

$$\frac{x^2}{17} - \frac{x^4}{18} + \frac{x^6}{19} - \frac{x^8}{20} + \frac{x^{10}}{21} - \frac{x^{12}}{22}, \text{ \&c.}$$
$$* \quad + \frac{2.x^4}{18} \quad * \quad + \frac{2.x^8}{20} \quad * \quad + \frac{2.x^{12}}{22} + \text{ \&c.}$$

Here, again, the value of the first series may easily be computed, by the theorem referred to in Art. 4, it being $= \frac{1}{17} \cdot \frac{x^2}{1+xx} + \frac{1}{17.18} \cdot \frac{x^4}{(1+xx)^2} + \frac{1.2}{17.18.19} \cdot \frac{x^6}{(1+xx)^3} + \frac{1.2.3}{17.18.19.20} \cdot \frac{x^8}{(1+xx)^4} +$ &c. in numbers,

$$P = \frac{1}{17} \times \frac{81}{181} = 0{\cdot}02632{,}43418{,}9$$

$$Q = \frac{P}{18} \times \frac{81}{181} = 0{\cdot}00065{,}44725{,}9$$

$$R = \frac{2Q}{19} \times \frac{81}{181} = 0{\cdot}00003{,}08300{,}6$$

$$S = \frac{3R}{20} \times \frac{81}{181} = 0{\cdot}00000{,}20695{,}3$$

$$T = \frac{4S}{21} \times \frac{81}{181} = 0{\cdot}00000{,}01764{,}1$$

$$U = \frac{5T}{22} \times \frac{81}{181} = 0{\cdot}00000{,}00179{,}4$$

$$V = \frac{6U}{23} \times \frac{81}{181} = 0{\cdot}00000{,}00020{,}9$$

$$W = \frac{7V}{24} \times \frac{81}{181} = 0{\cdot}00000{,}00002{,}7$$

$$X = \frac{8W}{25} \times \frac{81}{181} = 0{\cdot}00000{,}00000{,}4$$

And the sum of these terms is $0.02701{,}19108{,}2 = \mathfrak{E}$.

9. To find the value of $x^{32} \times : \frac{2x^4}{18} + \frac{2x^8}{20} + \frac{2x^{12}}{22} + \frac{2x^{16}}{24} +$ &c. *ad infinitum*, or its equal, $x^{32} \times : \frac{x^4}{9} + \frac{x^8}{10} + \frac{x^{12}}{11} + \frac{x^{16}}{12} +$ &c. *ad infinitum*, which is all that now remains of the proposed series, it will be expedient, first, to compute the two initial terms of the series, and then to separate the remaining terms of it into two parts, as has been done in the preceding articles. These two terms are

$$\frac{x^4}{9} = 0{\cdot}07290{,}00000{,}0$$

$$\frac{x^8}{10} = 0{\cdot}04304{,}67210{,}0$$

And their sum is $0{\cdot}11594{,}67210{,}0 = E$.

10. There now remains, of the proposed series, $x^{32} \times : \frac{x^{12}}{11} + \frac{x^{16}}{12} + \frac{x^{20}}{13} + \frac{x^{24}}{14} + \frac{x^{28}}{15} + \frac{x^{32}}{16} +$ &c. *ad infinitum*, $= x^{40} \times$ the sum of these two series,

$$\frac{x^4}{11} - \frac{x^8}{12} + \frac{x^{12}}{13} - \frac{x^{16}}{14} + \frac{x^{20}}{15} - \frac{x^{24}}{16}, \text{ \&c.}$$

$$* + \frac{2x^8}{12} \quad * + \frac{2x^{16}}{14} \quad * + \frac{2x^{24}}{16} + \text{ \&c.}$$

Here, likewise, the value of the series which has the signs + and − alternately, is easily obtained by a swiftly converging series, the terms of which are set down here below, the decimal value of $\frac{x^4}{1+x^4} = \frac{6561}{16561}$ being used in the calculation, for the sake of facility.

$$P = \frac{1}{11} \times 0{\cdot}39617{,}17287{,}6 = 0{\cdot}03601{,}56117{,}1$$

$$Q = \frac{P}{12} \times 0{\cdot}39617{,}17287{,}6 = 0{\cdot}00118{,}90306{,}0$$

$$R = \frac{2Q}{13} \times 0{\cdot}39617{,}17287{,}6 = 0{\cdot}00007{,}24708{,}2$$

$$S = \frac{3R}{14} \times 0{\cdot}39617{,}17287{,}6 = 0{\cdot}00000{,}61523{,}3$$

$$T = \frac{4S}{15} \times 0{\cdot}39617{,}17287{,}6 = 0{\cdot}00000{,}06499{,}7$$

$$U = \frac{5T}{16} \times 0{\cdot}39617{,}17287{,}6 = 0.00000{,}00804{,}7$$

$$V = \frac{6U}{17} \times 0{\cdot}39617{,}17287{,}6 = 0{\cdot}00000{,}00112{,}5$$

$$W = \frac{7V}{18} \times 0{\cdot}39617{,}17287{,}6 = 0.00000{,}00017{,}3$$

$$X = \frac{8W}{19} \times 0.39617{,}17287{,}6 = 0{\cdot}00000{,}00002{,}9$$

$$Y = \frac{9X}{20} \times 0{\cdot}39617{,}17287{,}6 = 0{\cdot}00000{,}00000{,}5$$

The sum of these terms is $0{\cdot}03728{,}40092{,}2 = \gamma.$

11. The part now remaining, of the proposed series, is $x^{40} \times : \frac{2x^8}{12} + \frac{2x^{16}}{14} + \frac{2x^{24}}{16} + \frac{2x^{32}}{18} + \frac{2x^{40}}{20} + \frac{2x^{48}}{22} +$ &c. *ad infinitum*, which may very conveniently be resolved into $x^{48} \times$ these two series

$$\text{series}\begin{cases} \frac{1}{6} + \frac{x^8}{7} - \frac{x^{16}}{8} + \frac{x^{24}}{9} - \frac{x^{32}}{10} + \frac{x^{40}}{11} - \frac{x^{48}}{12}, \text{ \&c.} \\ \quad\quad * + \frac{2x^{16}}{8} \quad * + \frac{2x^{32}}{10} \quad * + \frac{2x^{48}}{12} + \text{\&c.} \end{cases}$$

the value of the first of which will quickly be obtained, by means of the theorem which has been repeatedly used in the preceding operations. The terms of it are as below, in which the decimal value of $\frac{x^8}{1+x^8} = \frac{43046721}{143046721}$ is used, for the reason mentioned in the preceding article.

$$\frac{1}{6} = \quad - \quad - \quad 0{\cdot}16666,66667$$

$$P = \frac{1}{7} \times 0{\cdot}30092,77018 = 0{\cdot}04298,96717$$

$$Q = \frac{P}{8} \times 0{\cdot}30092,77018 = 0{\cdot}00161,70979$$

$$R = \frac{2Q}{9} \times 0{\cdot}30092,77018 = 0{\cdot}00010,81399$$

$$S = \frac{3R}{10} \times 0{\cdot}30092,77018 = 0{\cdot}00000,97627$$

$$T = \frac{4S}{11} \times 0{\cdot}30092,77018 = 0{\cdot}00000,10683$$

$$U = \frac{5T}{12} \times 0{\cdot}30092,77018 = 0{\cdot}00000,01339$$

$$V = \frac{6U}{13} \times 0.30092,77018 = 0{\cdot}00000,00186$$

$$W = \frac{7V}{14} \times 0{\cdot}30092,77018 = 0{\cdot}00000,00028$$

$$X = \frac{8W}{15} \times 0{\cdot}30092,77018 = 0{\cdot}00000,00005$$

$$Y = \frac{9X}{16} \times 0{\cdot}30092,77018 = 0{\cdot}00000,00001$$

The sum of these terms is $0{\cdot}21139,25631 = \delta$.

12. There now remains, of the proposed series, only $x^{-8} \times : \frac{2x^{16}}{8} + \frac{2x^{32}}{10} + \frac{2x^{48}}{12} + \frac{2x^{64}}{14} +$ &c. *ad infinitum*, the value of which, or of its equal, $x^{-8} \times : \frac{x^{16}}{4} + \frac{x^{32}}{5} + \frac{x^{48}}{6} + \frac{x^{64}}{7} +$ &c. may easily be obtained without any transformation, since the powers of x^{16} (= the powers of 0·18530,20188,9) decrease swifter than the powers of $\frac{1}{5}$. The numerical values of these terms are as below.

$$\frac{x^{16}}{4} = 0{\cdot}04632,55047$$
$$\frac{x^{32}}{5} = 0{\cdot}00686,73676$$
$$\frac{x^{48}}{6} = 0{\cdot}00106,04476$$
$$\frac{x^{64}}{7} = 0{\cdot}00016,84312$$
$$\frac{x^{80}}{8} = 0{\cdot}00002,73098$$
$$\frac{x^{96}}{9} = 0{\cdot}00000,44982$$
$$\frac{x^{112}}{10} = 0{\cdot}00000,07502$$
$$\frac{x^{128}}{11} = 0{\cdot}00000,01264$$
$$\frac{x^{144}}{12} = 0{\cdot}00000,00215$$
$$\frac{x^{160}}{13} = 0{\cdot}00000,00037$$
$$\frac{x^{176}}{14} = 0{\cdot}00000,00006$$
$$\frac{x^{192}}{15} = 0{\cdot}00000,00001$$

The sum of these terms is 0·05445,44611 = F.

13. The values of the several parts, into which the proposed series has been resolved, being now so far obtained that we have only to multiply each by its proper factor, *viz.* the numerical value of x^8, x^{16}, x^{32}, *&c.* and add the products together, to get the sum of it; this, therefore, is now to be performed. And, in this part of the calculation, several multiplications may be saved, and no larger factor than x^8 be used, by attending to the method described by Sir ISAAC NEWTON, in his Tract *De Analysi per Æquationes infinitas*; p. 10. of Mr. JONES'S edition of Sir ISAAC'S Tracts; or p. 270. Vol. I. of Bishop HORSLEY'S edition of all his works. The manner in which this is to be done will appear, by collecting the several parts from the preceding Articles, and exhibiting them in one view, thus:

$A + Bx^8 + Cx^{16} + \overline{\alpha + D} \times x^{24} + \overline{\beta + E} \times x^{32} + \gamma x^{40} + \overline{\delta + F} \times x^{48} =$ the sum of the proposed series. Now,

1st. Calling $\delta + F$, z', and multiplying by x^8, we have $z' x^8 = 0{\cdot}26584{,}70242 \times 0{\cdot}43046{,}721 = 0{\cdot}11443{,}84267{,}9$.

2dly. Putting $\gamma + z' x^8 = z''$, and multiplying by x^8, we get $z'' x^8 = 0{\cdot}15172{,}24360{,}1 \times 0{\cdot}43046{,}721 = 0{\cdot}06531{,}15337{,}2$.

3dly. Putting $\beta + E + z'' x^8 = z'''$, and multiplying by x^8, we get $z''' x^8 = 0{\cdot}20827{,}01655{,}4 \times 0{\cdot}43046{,}721 = 0{\cdot}08965{,}34770{,}9$.

4thly. Putting $\alpha + D + z''' x^8 = z^{iv}$, and multiplying by x^8, we get $z^{iv} x^8 = 0{\cdot}28046{,}43895{,}9 \times 0.43046{,}721 = 0{\cdot}12073{,}07232{,}90$.

5thly. Putting $C + z^{iv} x^8 = z^{v}$, and multiplying by x^8, we get $z^{v} x^8 = 0{\cdot}38085{,}19524{,}75 \times 0{\cdot}43046{,}721 = 0{\cdot}16394{,}42774{,}05$.

6thly. Putting $B + z^{v} x^8 = z^{vi}$, and multiplying by x^8, we get $z^{vi} x^8 = 0{\cdot}60806{,}50444{,}24 \times 0{\cdot}43046{,}721 = 0{\cdot}26175{,}20631{,}73$.

Lastly, to this product add A - $= 2{\cdot}04083{,}30298{,}21$, and we have the value of the series proposed $= 2{\cdot}30258{,}50929{,}94$, which is true to twelve places of figures.

14. It may not be improper to remark, that this degree of accuracy is much greater than is requisite, even in astronomy; for which, as well as for most other purposes, six or seven places of figures are sufficient: and, to that degree of exactness the value of the proposed series might have been obtained, by less than a fourth part of the labour that has been taken above. But it was my intention to show, that the value of a very slowly converging series, of which all the terms are affirmative, may, by the method now described, be computed to ten or twelve places of figures, in the space of a few hours.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January, 1797.

1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°										
Jan. 1	45	8	0	45	52	29,74	88		S	1	Cloudy.
	47	2	0	47	54	29,83	87		S	1	Cloudy.
2	43	8	0	43	53	29,91	89	0,025	S	2	Cloudy.
	45	2	0	45	56	29,98	87		SSE	1	Cloudy.
3	36	8	0	38	52	30,20	87		E	1	Fair.
	40	2	0	39	54	30,28	87		ESE	1	Cloudy.
4	34	8	0	36	52	30,33	87		E	1	Cloudy.
	39	2	0	39	54	30,35	86		E	1	Cloudy.
5	37	8	0	37	52	30,40	86		NE	1	Cloudy.
	39	2	0	39	53	30,40	81		NE	1	Cloudy.
6	32	8	0	32	51	30,43	81		NE	1	Cloudy.
	34	2	0	34	53	30,45	80		NE	1	Cloudy.
7	30	8	0	30	49	30,44	84		NE	1	Cloudy.
	31	2	0	31	51	30.45	85		NE	1	Cloudy.
8	29	8	0	29	48,5	30,47	82		NE	1	Cloudy.
	30	2	0	30	50	30,48	82		NE	1	Cloudy.
9	25	8	0	25	47	30,50	84		NE	1	Cloudy.
	26	2	0	26	48,5	30,46	85		NE	1	Cloudy.
10	25	8	0	31	46	30,18	87		NE	1	Snow.
	34	2	0	33	48	30,05	85		ENE	1	Cloudy.
11	26	8	0	27	45	29,80	80		E	1	Cloudy.
	30	2	0	29	48	29,70	79		ESE	2	Cloudy.
12	28	8	0	31	45	29,70	88		NE	1	Snow.
	32	2	0	32	47,5	29,84	85		NW	1	Snow.
13	25	8	0	25	45	30,03	80		W	1	Cloudy.
	42	2	0	36	47	29,84	86		SSE	2	Rain.
14	36	8	0	48	48	29,52	90	0,272	SW	2	Cloudy.
	49	2	0	42	50	29,61	88		NW	1	Cloudy.
15	35	8	0	36	48	29,78	88	0,308	SW	1	Cloudy.
	35	2	0	34	50	29,73	88		E	1	Rain.
16	32	8	0	33	48	30,01	87	0,244	NE	1	Cloudy.
	36	2	0	36	51	30,16	81		NE	1	Fine.

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1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°										
Jan. 17	25	8	0	26	48	30,39	87		SW	1	Fair.
	35	2	0	35	51	30,40	85		SSW	1	Fine.
18	31	8	0	35	48	30,34	85		SSW	1	Cloudy.
	39	2	0	38	50	30,29	87		SSW	1	Rain.
19	34	8	0	35	48	30,31	90	0,033	SSW	1	Foggy.
	40	2	0	39	51	30,35	90		SSW	1	Cloudy.
20	39	8	0	43	50	30,33	90		SSW	1	Cloudy.
	49	2	0	49	54	30,27	89		SSW	1	Cloudy.
21	47	8	0	47	53	30,26	90		SSW	1	Cloudy.
	48	2	0	48	55	30,20	86		SW	1	Cloudy.
22	43	8	0	43	54	30,16	87		SW	2	Cloudy.
	44	2	0	43	55,5	30,07	85		SSW	2	Cloudy.
23	43	8	0	43	53	30,07	87		S	2	Cloudy.
	43	2	0	43	55	30,04	85		S	2	Cloudy.
24	39	8	0	41	53	29,92	86		S	2	Fair.
	44	2	0	43	54,5	29,98	85		W	2	Cloudy.
25	30	8	0	30	52	30,24	86		SSW	1	Fair.
	38	2	0	38	54	30,24	83		SSW	1	Fine.
26	31	8	0	35	51,5	30,11	86		E	1	Cloudy.
	42	2	0	42	54,5	30,03	79		SE	1	Cloudy.
27	33	8	0	35	51	30,00	86		SE	1	Fair.
	45	2	0	42	54	29,89	87		SSE	1	Fair.
28	42	8	0	43	52	29,53	89	0,038	S	2	Rain. [Much wind last night.
	44	2	0	39	54	29,61	84		S	2	Cloudy.
29	36	8	0	43	51	29,92	87	0,040	SW	2	Rain.
	44,5	2	0	43	55	30,07	74		WNW	2	Fine.
30	38	8	0	42	52	30,17	85		WSW	2	Fair.
	49	2	0	49	55	30,16	82		WSW	2	Cloudy.
31	45	8	0	45,5	53	29,70	87		S	2	Rain. [Much wind last night.
	47	2	0	44	56	29,73	69		W	2	Fair.

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1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Feb. 1	36	7	0	44	53	29,96	88	0,068	SW	2	Cloudy.
	50	2	0	50	56	29,99	86		WNW	2	Cloudy.
2	46	7	0	46	54,5	30,27	87		WSW	2	Cloudy.
	50	2	0	50	57	30,32	85		SW	2	Cloudy.
3	45	7	0	45,5	56	30,37	82		SW	1	Cloudy.
	49	2	0	49	57	30,37	79		SSW	1	Cloudy.
4	37,5	7	0	38,5	56	30,36	85		NW	1	Cloudy.
	40	2	0	40	57	30,41	84		NNW	1	Cloudy.
5	35	7	0	35	55	30,50	84		E	1	Cloudy.
	36	2	0	36	55	30,54	85		E	1	Cloudy.
6	32	7	0	32,5	52	30,53	85		SE	1	Cloudy.
	37	2	0	37	54	30,55	82		NW	1	Cloudy.
7	32	7	0	33	52	30,55	82		SE	1	Cloudy.
	35	2	0	35	53	30,55	85		SE	1	Cloudy.
8	31	7	0	31	52	30,47	85		SW	1	Cloudy.
	35	2	0	32	52	30,48	83		SW	1	Cloudy.
9	32	7	0	35	51	30,59	82		SW	1	Cloudy.
	41	2	0	41	53	30,61	80		SSE	1	Fair.
10	31,5	7	0	31,5	52	30,62	83		SE	1	Cloudy.
	33	2	0	33	53	30,57	82		E	1	Cloudy.
11	29	7	0	30	51	30,44	84		S	1	Cloudy.
	36	2	0	36	52	30,33	85		SSE	1	Cloudy.
12	35	7	0	37	51	30,08	87		SW	1	Cloudy.
	46	2	0	46	52	30,04	85		SSW	1	Fair.
13	38	7	0	41	51	29,81	87	0,023	S	2	Rain.
	45	2	0	45	53	29,62	81		S	2	Cloudy.
14	36	7	0	36	51	29,44	84	0,128	WSW	2	Fair.
	42	2	0	42	53	29,37	76		WSW	2	Cloudy.
15	29	7	0	29,5	51	29,82	79		W	1	Fair.
	37	2	0	37	52	29,98	69		NW	2	Fine.
16	25	7	0	25	50	30,36	78		WNW	1	Fine.
	39	2	0	39	51	30,41	72		W	1	Fair.

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1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Feb. 17	27,5	7	0	27,5	50	30,46	78		E	1	Fine.
	41,5	2	0	41,5	54	30,40	73		ESE	1	Fine.
18	29	7	0	30	51	30,40	82		E	1	Fair.
	46	2	0	46	54	30,40	70		SSE	1	Fine.
19	29	7	0	29	51	30,48	81		ENE	1	Fair.
	44	2	0	43,5	54,5	30,50	80		S	1	Fine.
20	28	7	0	28	52	30,43	81		NE	1	Fair.
	44	2	0	44	55	30,37	77		EN E	1	Fine.
21	26	7	0	26	51,5	30,40	82		EN E	1	Foggy.
	46	2	0	46	55	30,40	82		E	1	Fine.
22	30	7	0	33	51,5	30,42	83		E	1	Fair.
	49,5	2	0	49,5	56	30,44	78		SE	1	Fine.
23	31	7	0	31	51	30,42	84		NE	1	Cloudy.
	49	2	0	46,5	54	30,41	81		NE	1	Foggy.
24	29	7	0	29	54	30,39	81		S	1	Fair.
	46	2	0	45	55	30,38	81		S	1	Foggy.
25	31,5	7	0	32	54	30,36	80		E	1	Fair.
	50	2	0	50	56	30,34	80		E	1	Fine.
26	34	7	0	35	54	30,31	84		E	1	Cloudy.
	47	2	0	47	55	30,31	83		E	1	Fair.
27	32,5	7	0	33,5	54	30,41	77		NE	1	Cloudy.
	43	2	0	42	56	30,41	67		E	1	Fine.
28	24,5	7	0	25	53	30,25	83		NE	1	Foggy.
	42	2	0	41,5	56	30,15	75		E	1	Fine.

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1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Mar. 1	31	7	0	31	53	30,02	81		ENE	1	Fair.
	40	2	0	39	56	29,98	76		ENE	2	Fine.
2	28	7	0	31	53	29,92	83		E	1	Cloudy.
	44	2	0	44	55	29,90	77		E	1	Fine.
3	32	7	0	33	53,5	29,83	81		E	1	Fair.
	53	2	0	53	57	29,81	69		SSE	1	Fine.
4	35	7	0	35	55	29,72	78		E	1	Fine.
	44	2	0	43,5	55	29,69	74		E	2	Hazy.
5	33	7	0	33	53	29,60	77		E	2	Fair.
	41,5	2	0	41	57	29,54	71		E	2	Fair.
6	36	7	0	35	53	29,71	84		NE	2	Cloudy.
	45	2	0	45	56	29,86	81		WNW	2	Cloudy.
7	33	7	0	33	53	30,15	85		SW	1	Foggy.
	48,5	2	0	48	57	30,19	74		S	1	Fair.
8	36	7	0	36	54	30,02	86	0,335	NE	1	Rain.
	42	2	0	41,5	56	30,03	85		N	1	Cloudy.
9	34	7	0	35	54	30,03	86	0,176	E	1	Cloudy.
	42	2	0	41	55	30,05	78		NE	1	Cloudy.
10	31,5	7	0	33	53	29,86	80		NE	2	Cloudy.
	38,5	2	0	37	55	29,84	74		NE	2	Cloudy.
11	32	7	0	32,5	52	29,78	76		NE	2	Cloudy.
	42	2	0	42	54	29,83	70		NE	2	Fair.
12	32	7	0	32	52	30,00	74		NE	2	Cloudy.
	38	2	0	38	53	30,00	71		NE	2	Fair.
13	31,5	7	0	31,5	51	30,08	75		ENE	2	Cloudy.
	40,5	2	0	40	53	30,12	71		NE	2	Fair.
14	33	7	0	32	51,5	30,11	77		NE	2	Cloudy.
	45	2	0	45	54	30,05	75		NE	2	Cloudy.
15	35,5	7	0	35,5	52	30,03	79		ENE	1	Cloudy.
	47	2	0	46,5	54	30,02	72		E	2	Fine.
16	30,5	7	0	32	52	29,98	83		NE	2	Cloudy.
	47	2	0	47	54	29,97	70		NE	2	Cloudy.

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		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°										
Mar. 17	34	7	0	34	53	30,03	75		NE	2	Cloudy.
	42	2	0	42	53	30,06	70		NE	2	Cloudy.
18	31	7	0	33	53	30,16	79		NE	2	Cloudy.
	43	2	0	43	54	30,18	69		NE	2	Cloudy.
19	34	7	0	34	53	30,16	74		NE	1	Cloudy.
	44	2	0	44	54	30,20	70		NE	1	Cloudy.
20	32,5	7	0	35	52	30,20	82		NE	1	Cloudy.
	45	2	0	45	54	30,28	72		NE	1	Cloudy.
21	27,5	7	0	29	52	30,42	75		NE	1	Cloudy.
	43	2	0	43	57	30,42	67		E	1	Fair.
22	27,5	7	0	29,5	52	30,42	75		SE	1	Fair.
	47	2	0	47	56	30,38	64		S b. E	1	Fair.
23	31	7	0	32	52	30,24	73		SSW	1	Fair.
	54	2	0	53	54	30,11	60		SSW	2	Hazy.
24	38	7	0	39	54	30,06	84		SSW	1	Cloudy.
	54	2	0	54	56	29,94	74		SSW	2	Cloudy.
25	45	7	0	45	54	29,66	86		S	2	Cloudy.
	52	2	0	52	56	29,61	75		W	2	Cloudy.
26	38	7	0	42	55	29,82	82	0,076	SW	2	Cloudy.
	49	2	0	48	57	29,70	76		SSW	2	Cloudy.
27	43	7	0	44	55	29,54	84	0,068	SE	1	Cloudy.
	50	2	0	49	56	29,49	80		SE	2	Rain.
28	40	7	0	42	55	29,44	84	0,042	SE	2	Cloudy.
	51	2	0	51	57	29,49	72		SE	2	Cloudy.
29	38	7	0	39	55	29,55	82		E	1	Foggy.
	54	2	0	53,5	59	29,60	72		SW	1	Fair.
30	37	7	0	39	56	29,76	81		SW	1	Cloudy.
	44	2	0	44	57	29,80	82		NE	1	Cloudy.
31	34	7	0	36	56	29,85	83	0,080	SW	1	Fair.
	48	2	0	48	57	29,82	77		SSW	2	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	o	o	Inches.		Inches.	Points.	Str.	
	o										
Apr. 1	41	7	0	42	57	29,75	84		S	2	Cloudy.
	52	2	0	51	58	29,70	75		W	2	Cloudy.
2	41	7	0	41	57	29,55	83	0,061	SW	2	Cloudy.
	52	2	0	52	58	29,45	81		W	2	Cloudy.
3	35	7	0	35	56	29,10	84	0,210	NE	1	Rain.
	39	2	0	39	57	29,13	84		NE	1	Rain.
4	34	7	0	37	55	29,23	84	0,520	E	1	Fine.
	51	2	0	50	57	29,40	70		E	1	Hazy.
5	40	7	0	40	55	29,55	85		NE	1	Cloudy.
	44	2	0	44	56	29,53	80		NE	1	Cloudy.
6	36,5	7	0	38	55	29,57	82		NE	1	Cloudy.
	47	2	0	47	56	29,57	76		NE	1	Cloudy.
7	37	7	0	37	55	29,68	83		NE	1	Cloudy.
	48	2	0	48	57	29,75	77		NE	1	Cloudy.
8	40	7	0	40	56	29,97	84		E	1	Cloudy.
	48	2	0	48	57	29,98	81		NE	1	Cloudy.
9	38	7	0	39	55	30,12	83		NE	1	Cloudy.
	50	2	0	50	57	30,11	76		NE	2	Cloudy.
10	40	7	0	42	55	30,07	82		NE	2	Cloudy.
	47	2	0	44	57	29,97	77		NE	2	Cloudy.
11	40	7	0	41	55	29,77	81		NE	2	Cloudy.
	50	2	0	46	57	29,76	79		NE	2	Cloudy.
12	40,5	7	0	42,5	56	29,82	86		NE	1	Cloudy.
	63	2	0	61,5	59	29,80	72		ESE	1	Cloudy.
13	45	7	0	46	58	29,95	84		NE	1	Cloudy.
	54	2	0	54	59	29,89	79		NE	1	Cloudy.
14	42,5	7	0	43	58	29,94	85	0,397	NE	1	Rain.
	48	2	0	48	58	29,93	83		E	1	Cloudy.
15	42,5	7	0	44	58	29,87	87	0,052	E	1	Cloudy.
	56	2	0	55	60	29,83	69		S	1	Cloudy.
16	42	7	0	43	58	29,79	80		NW	1	Cloudy.
	56	2	0	54	60	29,79	73		W	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Apr. 17	38	7	0	40	58	29,77	84		W	1	Fair.
	58	2	0	58	59	29,76	65		W	1	Cloudy.
18	40	7	0	42	57	29,78	70		NW	1	Fair.
	56	2	0	56	59	29,80	63		SW	1	Fair.
19	37	7	0	42	58	29,93	77		NE	1	Fair.
	56	2	0	55	61	29,96	65		NE	1	Fine.
20	36	7	0	40	58	30,13	75		E	1	Fine.
	57	2	0	57	60	30,08	65		E	1	Fine.
21	40	7	0	43	58	29,89	80		E	1	Cloudy.
	59	2	0	59	59	29,83	80		E	1	Cloudy.
22	46	7	0	47	58	29,77	83		E	1	Cloudy.
	61	2	0	61	61	29,78	68		E	1	Fair.
23	45	7	0	47	59	29,94	82		S	1	Fine.
	64	2	0	63	61	29,94	64		S	1	Fair.
24	47	7	0	51	60	29,94	77		SSE	1	Fair.
	65	2	0	65	61	29,94	64		SSW	1	Cloudy.
25	45	7	0	46	60	30,00	82		SW	1	Cloudy.
	62	2	0	61	62	30,03	67		SSW	1	Fair.
26	42	7	0	47	60	30,05	76		SW	1	Fair.
	56	2	0	51	58	29,86	71		SSE	1	Rain.
27	44	7	0	45	59	29,46	82	0,394	NW	2	Cloudy.
	53	2	0	51	60	29,52	78		NW	2	Cloudy.
28	42	7	0	43	59	29,74	79		NE	2	Cloudy.
	55,5	2	0	55	61	29,75	71		NE	1	Fair.
29	42	7	0	47	60	29,65	80		S	1	Cloudy.
	57	2	0	55	61	29,52	76		SSW	1	Rain.
30	44	7	0	44	59	29,38	81	0,225	SSW	1	Cloudy.
	58	2	0	57	60	29,39	67		NW	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds. Points.	Winds. Str.	Weather.
	o			o	o	Inches.		Inches.			
May 1	43	7	0	44	58	29,54	77		NW	1	Cloudy.
	57	2	0	54	61	29,66	71		NW	1	Fair.
2	42	7	0	48	59	29,76	84	0,022	S	2	Rain.
	57	2	0	55	59	29,64	80		S	2	Rain.
3	42	7	0	46	59	29,61	82	0,202	SSW	2	Fine.
	58	2	0	56	61	29,62	73		SW	2	Fair.
4	43	7	0	46	59	29,56	83	0,072	S	2	Rain.
	53	2	0	51	61	29,38	84		S	2	Rain.
5	39	7	0	45	58	29,49	76	0,198	SSW	2	Cloudy.
	56	2	0	52	61	29,46	75		S	2	Rain.
6	41	7	0	45	59	29,70	80	0,160	SW	1	Fine.
	57	2	0	55	60	29,79	73		NW	1	Cloudy.
7	42	7	0	44	59	29,91	82	0,196	E	1	Cloudy.
	47	2	0	47	60	29,91	80		NE	1	Cloudy.
8	37	7	0	42	58	30,06	75	0,070	NE	1	Fair.
	52	2	0	52	61	30,03	69		NE	1	Fair.
9	36	7	0	41	58	29,88	79		NE	2	Cloudy.
	49	2	0	48	58	29,77	77		NE	2	Cloudy.
10	34	7	0	40	56	29,61	90	0,265	NE	2	Rain.
	49	2	0	47	58	29,61	84		NE	2	Cloudy.
11	41,5	7	0	42	56	29,58	85		NE	1	Cloudy.
	50	2	0	50	57	29,55	77		NE	1	Cloudy.
12	42	7	0	45	57	29,58	82		E	1	Cloudy.
	60	2	0	59	59	29,58	70		E	1	Fair.
13	42	7	0	46	58	29,88	78	0,068	SSW	2	Fair.
	60	2	0	59	61	29,98	65		S	2	Fair.
14	42	7	0	47	58	30,17	73		NW	1	Fair.
	61	2	0	61	60	30,19	66		E	1	Hazy.
15	43	7	0	47	59	30,08	80		E	1	Cloudy.
	62	2	0	61,5	61	30,00	71		E	1	Fine.
16	47	7	0	53	60	29,91	81		E	1	Fair.
	67	2	0	67	62	29,85	74		E	1	Fair.

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1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°										
May 17	53	7	0	56	61	29,86	76		S	2	Cloudy.
	63	2	0	61	62	29,80	83		S	2	Rain.
18	52	7	0	54	62	29,91	83	0,115	S	2	Cloudy.
	68	2	0	67	62	29,96	77		SSW	2	Cloudy.
19	51	7	0	55	62	29,99	81		S	1	Cloudy.
	73	2	0	71	65	29,90	72		ESE	2	Fair.
20	59	7	0	61	64	29,67	76		SSW	2	Cloudy.
	68	2	0	68	65	29,77	67		SSW	2	Fair.
21	52	7	0	54	63	29,91	76		SSW	2	Fair.
	68	2	0	68	64	29,95	66		SSW	2	Fine.
22	49	7	0	52	63	30,08	75		SSW	2	Fair.
	66	2	0	66	64	30,17	61		SW	1	Fine.
23	48	7	0	53	63	30,33	72		W	1	Hazy.
	69	2	0	68	64	30,33	65		SE	1	Fair.
24	48	7	0	54	64	30,28	75		E	1	Fair.
	71	2	0	70	65	30,20	63		E	2	Fine.
25	50	7	0	58	64	30,08	73		E	2	Fair.
	79	2	0	78	68	30,03	68		SSE	2	Fine.
26	61	7	0	64	68	29,95	76		W	1	Fair.
	73	2	0	73	68	29,96	66		SSW	1	Cloudy.
27	50	7	0	54	66	30,09	75		SSW	2	Fair.
	71	2	0	71	67	30,09	65		SW	2	Fair.
28	50	7	0	54	65	29,98	76		S	1	Cloudy.
	70	2	0	70	66	29,83	66		SSE	2	Fair.
29	51	7	0	54	65	29,70	74		SSW	2	Fair.
	62	2	0	62	64	29,59	76		S	2	Rain.
30	49	7	0	50	63	29,77	81	0,068	NE	2	Cloudy.
	60	2	0	59	62	29,99	72		NE	2	Cloudy.
31	46	7	0	53	62	30,10	74		SSW	2	Hazy.
	65	2	0	65	62	30,00	69		SW	2	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Str.	Weather.
	°			°	°						
June 1	52	7	0	54	62	29,71	83		SW	2	Cloudy.
	65	2	0	64	62	29,67	75		SW	2	Cloudy.
2	49	7	0	53	62	29,78	71	0,295	E	1	Cloudy.
	69	2	0	69	63	29,76	66		S	1	Cloudy.
3	51	7	0	54	62	29,36	85	0,570	SSW	1	Rain.
	56	2	0	48	61	29,52	80		WNW	1	Rain.
4	40	7	0	45	60	29,82	73	0,225	W	2	Fine.
	63	2	0	63	60	29,90	66		W	2	Cloudy.
5	46	7	0	50	60	30,04	73		NE	1	Fair.
	63	2	0	62	61	30,14	64		NE	1	Fine.
6	43	7	0	46	60	30,29	76		NE	1	Fine.
	64	2	0	63	61	30,27	65		NE	1	Fine.
7	45	7	0	53	60	30,01	78		W	1	Cloudy.
	62	2	0	58	61	29,89	80		NW	1	Rain.
8	45	7	0	50	60	29,78	75		SW	1	Fine.
	64	2	0	63	60	29,71	65		SW	1	Fair.
9	46	7	0	49	59	29,75	78	0,210	NE	1	Cloudy.
	61	2	0	60	61	29,84	70		E	1	Cloudy.
10	47	7	0	53	59	30,06	77	0,060	NE	2	Fair.
	62	2	0	60	60	30,18	70		NE	2	Fair.
11	44	7	0	52	60	29,98	73		N	1	Fine.
	69	2	0	66	60	29,88	66		N	1	Cloudy.
12	53	7	0	55	60	29,66	81	0,050	SW	1	Fair.
	65	2	0	60	61	29,66	77		W	1	Rain.
13	49	7	0	53	61	29,68	80	0,218	E	1	Cloudy.
	65	2	0	65	62	29,68	68		E	1	Fair.
14	50	7	0	54	61	29,74	78		W	1	Cloudy.
	69	2	0	68	62	29,74	70		W	1	Fair.
15	51	7	0	53	62	29,76	82	0,045	W	1	Cloudy.
	64	2	0	64	62	29,80	79		N	1	Cloudy.
16	50	7	0	53	62	30,04	78	0,185	NE	1	Cloudy.
	66	2	0	65	62	30,08	69		NE	1	Cloudy.

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	°			°	°	Inches.		Inches.			
June 17	53	7	0	55	62	30,16	75		S	1	Fair.
	71	2	0	69	63	30,16	70		SW	1	Cloudy.
18	51	7	0	57	62	30,15	75		E	1	Fine.
	73	2	0	73	63	30,10	67		E	1	Fair.
19	49	7	0	56	62	29,94	76		E	1	Fine.
	71	2	0	70	63	29,84	66		E	1	Cloudy.
20	52	7	0	56	63	29,77	80		E	1	Fair.
	71	2	0	68	65	29,71	70		ESE	1	Cloudy.
21	51	7	0	54	63	29,68	74	0,673	E	1	Cloudy.
	65	2	0	63	64	29,64	74		E	1	Cloudy.
22	53	7	0	55	63	29,54	81	0,088	E	1	Cloudy.
	67	2	0	63	63	29,44	69		ESE	2	Cloudy.
23	51	7	0	53	63	29,34	84	0,280	ESE	1	Rain.
	63	2	0	62	63	29,39	81		W	1	Cloudy.
24	52	7	0	54	62	29,75	82	0,535	SW	1	Cloudy.
	65	2	0	65	63	29,84	76		WSW	1	Cloudy.
25	52	7	0	53	62	30,03	83	0,570	SW	1	Rain.
	69	2	0	68	63	30,04	66		SW	1	Fair.
26	50	7	0	53	62	30,08	80		SW	1	Fair.
	66	2	0	65	64	30,07	68		SW	1	Fair.
27	50	7	0	53	62	30,02	81	0,022	N	1	Cloudy.
	67	2	0	65	64	29,93	71		NE	1	Cloudy.
28	51	7	0	54	62	29,99	79		E	1	Cloudy.
	70	2	0	70	64	30,00	65		SE	1	Fair.
29	50	7	0	53	63	29,87	82	0,135	W	1	Rain.
	66	2	0	64	63	29,82	67		NE	1	Fair.
30	50	7	0	54	63	30,06	77	0,062	NE	1	Cloudy.
	67	2	0	64	63	30,10	69		SE	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
July 1	54	7	0	56	63	30,07	78		SW	1	Cloudy.
	70	2	0	68	63	30,01	74		S	1	Cloudy.
2	51	7	0	56	63	30,00	81		SW	1	Cloudy.
	72	2	0	69	64	30,03	71		SW	2	Fair.
3	54	7	0	55	63	29,91	80		SW	2	Cloudy.
	64	2	0	61	63	29,74	80		S	2	Cloudy.
4	48	7	0	55	63	29,91	78	0,101	W	2	Fair.
	69	2	0	67	64	29,94	69		W	2	Cloudy.
5	54	7	0	55	63	29,76	80	0,020	SSW	2	Rain.
	65	2	0	63	63	29,64	83		SSW	2	Cloudy.
6	51	7	0	56	62	29,51	79	0,063	SSW	2	Cloudy.
	66	2	0	64	63	29,51	74		WNW	2	Cloudy.
7	52	7	0	57	62	29,72	79		W	2	Cloudy.
	68	2	0	65	62	29,76	71		W	1	Cloudy.
8	54	7	0	57	63	29,77	80		NW	1	Cloudy.
	67	2	0	66	63	29,88	75		NW	1	Cloudy.
9	54	7	0	58	63	30,14	78		NE	1	Fine.
	75	2	0	74	64	30,19	68		N	1	Cloudy.
10	58	7	0	60	64	30,25	82		SW	1	Cloudy.
	81	2	0	78	66	30,24	72		S	1	Fair.
11	59	7	0	61	66	30,13	80	0,082	S	1	Fine.
	80	2	0	79	68	30,03	67		S	1	Fine.
12	57	7	0	59	66	29,92	78		S	2	Cloudy.
	74	2	0	73	67	29,92	70		S	2	Cloudy.
13	55	7	0	58	66	29,92	77		SE	1	Fair.
	76	2	0	75	68	29,88	69		SE	1	Fine.
14	61	7	0	66	68	29,84	82	0,110	E	1	Fine.
	85	2	0	84	69	29,87	70		SE	1	Fine.
15	60	7	0	66	69	29,90	74		E	1	Cloudy.
	82	2	0	81	71	29,92	68		E	1	Hazy.
16	59	7	0	64	70	30,04	79		SW	1	Fine.
	85	2	0	84	73	29,99	67		SSW	1	Fair.

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	°			°	°	Inches.		Inches.			
July 17	62	7	0	65	71	29,95	77	0,420	S	1	Fine. Much thunder and lightning last night.
	84	2	0	83	73	29,96	66		S	1	Fine.
18	56	7	0	61	71	30,07	75		S	1	Fine.
	76	2	0	76	72	30,07	64		SW	1	Fair.
19	57	7	0	60	70	30,07	77		SSW	2	Cloudy.
	73	2	0	73	70	30,02	68		SSW	2	Fair.
20	59	7	0	59	69	30,00	81	0,125	S	1	Rain.
	71	2	0	69	69	29,98	76		SSW	1	Fair.
21	51	7	0	56	68	30,11	78	0,102	SW	1	Fair.
	71	2	0	71	69	30,14	66		SW	1	Fair.
22	51	7	0	55	66	30,16	77		W	1	Cloudy.
	63	2	0	63	66	30,15	75		W	1	Cloudy.
23	50	7	0	56	64	30,21	81		W	1	Fine.
	75	2	0	75	68	30,23	66		W	1	Fair.
24	55	7	0	63	66	30,20	78		W	1	Cloudy.
	80	2	0	79	69	30,13	66		W	1	Fine.
25	56	7	0	59	67	30,11	82		SW	1	Cloudy.
	80	2	0	79	73	30,10	71		W	1	Fine.
26	60	7	0	63	69	30,07	80		SW	1	Fine.
	85	2	0	83	73	30,04	66		SSW	1	Fine.
27	63	7	0	66	71	29,98	75		W	1	Hazy.
	80	2	0	80	74	29,99	68		W	1	Fair.
28	61	7	0	64	69	30,04	74		N	1	Hazy.
	75	2	0	74	72	30,04	68		SSE	1	Hazy.
29	58	7	0	65	71	29,93	80		E	1	Cloudy.
	78	2	0	76	73	29,86	70		E	1	Fair.
30	62	7	0	62	72	29,61	87	0,265	NE	1	Rain.
	76	2	0	75	72	29,69	72		S	2	Fair.
31	59	7	0	62	68	29,74	77		S	2	Cloudy.
	73	2	0	73	70	29,74	69		S	2	Fair.

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	°			°	°	Inches.		Inches.			
Aug. 1	58	7	0	58	69	29,72	84	0,123	SE	1	Rain.
	72	2	0	70	69	29,60	76		S	2	Cloudy.
2	53	7	0	57	68	29,76	80	0,142	SSW	1	Fine.
	70	2	0	65	69	29,74	74		SSW	1	Rain.
3	54	7	0	58	67	29,74	77	0,190	SSW	2	Cloudy.
	69	2	0	66	67	29,74	78		SW	2	Rain.
4	53	7	0	54	66	29,81	81	0,133	SSW	2	Cloudy.
	71	2	0	69,5	67	29,85	68		W	1	Fair.
5	55	7	0	57	67	29,97	79	0,080	W	1	Fair.
	71	2	0	65	67	29,99	71		W	1	Rain.
6	50	7	0	53	66	30,09	83	0,083	SSW	1	Fine.
	70	2	0	68	67	30,10	73		SSW	1	Fair.
7	51	7	0	55	65	30,09	78	0,038	SSW	1	Fine.
	70	2	0	70	66	30,09	66		SSW	1	Cloudy.
8	55	7	0	58	65	29,93	78		ESE	1	Fair.
	76	2	0	76	66	29,79	67		SSE	2	Fine.
9	55	7	0	57	67	29,74	73		SW	2	Fair.
	71	2	0	69	67	29,81	68		SW	2	Cloudy.
10	54	7	0	57	66	29,93	77	0,030	SW	1	Cloudy.
	69,5	2	0	68	66	29,94	70		SW	1	Cloudy.
11	58	7	0	58	66	29,72	80		SSW	1	Rain.
	64	2	0	63	66	29,68	80		S	2	Cloudy.
12	50	7	0	53	65	29,78	80	0,057	S	1	Fine.
	72	2	0	71	67	29,78	68		SW	1	Cloudy.
13	49	7	0	53	65	29,78	78		SW	1	Cloudy.
	70	2	0	69	65	29,81	66		SW	2	Cloudy.
14	53	7	0	56	65	29,90	82	0,113	SW	1	Fine.
	73	2	0	73	66	29,98	67		SW	1	Fair.
15	53	7	0	57	65	29,96	75		S	1	Cloudy.
	71	2	0	69	65	29,88	73		S	2	Cloudy.
16	56	7	0	57	66	29,77	83	0,110	NE	1	Cloudy.
	68	2	0	68	65	29,83	75		S	1	Cloudy.

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	°	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Aug. 17	51	7	0	55	65	29,97	80		S	1	Fine.
	72	2	0	71	67	29,91	70		SE	1	Fair.
18	58	7	0	58	66	29,48	88	0,195	E	1	Cloudy.
	72	2	0	69	66	29,60	69		S	2	Fair.
19	55	7	0	58	67	29,66	82		SSE	1	Fair.
	70	2	0	70	67	29,64	68		S	1	Fair.
20	53	7	0	58	66	29,85	81	0,170	SW	1	Cloudy.
	66	2	0	61	66	29,84	80		S	1	Rain.
21	54	7	0	56	65	29,86	80	0,156	SW	2	Fair.
	70	2	0	69	68	29,87	69		WSW	2	Fine.
22	48	7	0	53	63	30,07	78		WSW	1	Fine.
	67	2	0	67	66	30,10	67		NW	1	Cloudy.
23	50	7	0	52	64	30,14	81		NW	1	Cloudy.
	67	2	0	67	65	30,14	73		NW	1	Cloudy.
24	52	7	0	56	64	30,18	81	0,025	W	1	Cloudy.
	71	2	0	70	65	30,18	70		SW	1	Cloudy.
25	56	7	0	58	64	30,04	83	0,074	SE	1	Cloudy.
	65	2	0	64	65	30,01	78		S	1	Cloudy.
26	48	7	0	52	64	30,02	82		S	1	Fine.
	71	2	0	71	66	29,95	73		S	1	Fair.
27	60	7	0	61	65	29,83	86	0,150	S	1	Rain.
	72	2	0	72	67	29,83	75		S	2	Fair.
28	59	7	0	61	66	29,80	86	0,095	S	2	Rain.
	70	2	0	70	66	29,80	81		S	2	Cloudy.
29	57	7	0	62	66	29,88	80		S	2	Cloudy.
	72	2	0	71	67	29,81	78		S	2	Cloudy.
30	59	7	0	59	67	29,70	86	0,825	S	1	Fair.
	69	2	0	69	67	29,77	70		S	1	Fair.
31	52	7	0	57	66	29,90	82		SSE	1	Cloudy.
	70	2	0	70	66	29,86	71		S	1	Cloudy.

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1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Sept. 1	57	7	0	60	66	29,53	86	0,075	E	1	Cloudy.
	71	2	0	69	67	29,45	78		SE	2	Cloudy.
2	53	7	0	53	66	29,61	84	0,840	WNW	1	Rain.
	62	2	0	62	66	29,80	75		WNW	1	Cloudy.
3	44	7	0	47	63	30,10	80		SW	1	Fine.
	64	2	0	64	64	30,13	67		NW	1	Cloudy.
4	45	7	0	52	64	30,13	77		E	1	Cloudy.
	64	2	0	64	64	30,11	67		SW	1	Cloudy.
5	44	7	0	46	64	30,14	80		W	1	Fine.
	68	2	0	68	64	30,07	67		W	1	Fair.
6	54	7	0	55	62	29,83	81	0,112	S	2	Rain.
	68	2	0	68	63	29,79	78		SW	2	Cloudy.
7	55	7	0	55	63	29,74	89	0,550	S	2	Cloudy.
	69	2	0	69	64	29.70	70		SW	2	Fair.
8	51	7	0	52	64	29,84	77		W	1	Cloudy.
	63	2	0	63	64	29,84	66		SSW	1	Fair.
9	47	7	0	49	63	29,67	77	0,145	SW	2	Fine.
	62	2	0	62	63	29,84	65		W	2	Fair.
10	42	7	0	45	62	30,07	78		W	1	Fine.
	62	2	0	62	63	29,98	70		W	1	Fair.
11	50	7	0	51	62	29,37	80		E	1	Rain.
	59	2	0	58	62	29,04	90		E	1	Rain.
12	50	7	0	51	61	29,51	81	0,505	SSW	2	Fine.
	60	2	0	58	61	29,57	73		SW	2	Fair.
13	52	7	0	53	61	29,80	83	0,030	SSW	2	Fine.
	66	2	0	64	63	29,73	71		S	2	Fair.
14	55	7	0	57	62	29,61	82	0,275	S	2	Cloudy.
	62	2	0	62	62	29,54	78		S	2	Cloudy.
15	48	7	0	50	61	29,78	81	0,088	S	1	Fine.
	64	2	0	64	61	29,96	75		W	1	Fair.
16	47	7	0	49	61	30,13	82		SSW	1	Fair.
	64	2	0	64	61	30,05	73		S	2	Cloudy.

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for September, 1797.

1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Sept. 17	55	7	0	58	61	29,67	90	0,203	SW	2	Cloudy.
	68	2	0	68	62	29,73	78		SW	1	Cloudy.
18	51	7	0	52	61	29,62	82	0,050	SSW	1	Fine.
	64	2	0	64	62	29,62	74		SSW	1	Fair.
19	49	7	0	52	61	29,61	82	0,032	E	1	Cloudy.
	60	2	0	57	61	29,43	81		E	1	Rain.
20	49	7	0	51	60	29,41	83	0,390	S	2	Cloudy.
	59	2	0	57	61	29,43	76		S	2	Fair.
21	46	7	0	47	60	29,72	85		S	1	Fine.
	63	2	0	63	62	29,79	70		S	1	Fine.
22	42	7	0	43	60	29,89	84		E	1	Hazy.
	61	2	0	61	61	29,91	73		E	1	Cloudy.
23	50	7	0	51	60	29,80	88		NE	1	Cloudy.
	65	2	0	65	62	29,71	75		E	1	Fine.
24	55	7	0	55	61	29,60	87		SW	1	Cloudy.
	62	2	0	62	62	29,57	80		E	1	Cloudy.
25	53	7	0	53	61	29,55	88	0,270	N	1	Cloudy.
	60	2	0	60	62	29,64	87		N	1	Rain.
26	55	7	0	55	61	29,75	90	0,410	NE	1	Cloudy.
	63	2	0	62	62	29,71	84		NE	1	Cloudy.
27	53	7	0	53	61	29,69	86		ESE	1	Cloudy.
	63	2	0	62	62	29,73	79		E	1	Cloudy.
28	52	7	0	54	61	29,90	87		NE	1	Foggy.
	64	2	0	64	62	29,94	81		NE	1	Hazy.
29	54	7	0	55	62	29,96	88		E	1	Cloudy.
	65	2	0	64	62	29,86	79		S	1	Cloudy.
30	53	7	0	54	61	29,54	88	0,085	S	2	Rain.
	61	2	0	60	61	29,58	72		S	2	Cloudy.

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for October, 1797.

1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Oct. 1	50	7	0	52	61	29,85	84		SW	1	Cloudy.
	57	2	0	57	61	29,96	75		W	1	Fair.
2	46	7	0	46	61	30,02	84		E	1	Fair.
	62	2	0	62	61	30,00	77		S	1	Cloudy.
3	53	7	0	54	61	29,99	86	0,063	E	1	Fair.
	63	2	0	62	61	29,96	75		SE	1	Fair.
4	53	7	0	53	61	30,04	86		NE	1	Cloudy.
	61	2	0	61	62	30,08	77		NE	1	Fair.
5	50	7	0	51	61	30,12	86		NE	1	Cloudy.
	59	2	0	59	60	30,09	82		NE	1	Cloudy.
6	52	7	0	52	61	29,97	87	0,065	NE	1	Cloudy.
	57	2	0	57	60	29,91	84		NE	1	Rain.
7	44	7	0	45	60	29,93	83		NE	1	Fine.
	58	2	0	57	60	29,95	82		NE	1	Fair.
8	44	7	0	46	59	30,00	84		W	1	Cloudy.
	61	2	0	60	60	29,96	82		S	2	Cloudy.
9	51	7	0	52	59	30,02	87		WSW	1	Cloudy.
	61	2	0	60	60	30,07	73		NNE	1	Fair.
10	49	7	0	50	59	30,27	73		NE	1	Cloudy.
	58	2	0	58	58	30,28	67		NE	1	Cloudy.
11	50	7	0	50	58	30,31	80		NW	1	Cloudy.
	57	2	0	57	58	30,31	84		NW	1	Cloudy.
12	46	7	0	49	58	30,31	83		NW	1	Cloudy.
	57	2	0	57	58	30,25	78		NW	1	Cloudy.
13	48	7	0	48	58	30,25	82		NW	1	Fair.
	57	2	0	57	58	30,14	72		NW	1	Fair.
14	46	7	0	47	58	29,93	81	0,035	NW	1	Fine.
	53	2	0	52	57	30,08	73		NW	1	Fine.
15	39	7	0	42	57	30,19	81		SW	1	Cloudy.
	58	2	0	57	57	30,16	79		SW	1	Cloudy.
16	48	7	0	51	57	30,14	85		S	1	Cloudy.
	58	2	0	58	58	30,12	78		SSW	1	Cloudy.

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for October, 1797.

1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°										
Oct. 17	50	7	0	53	57	30,03	85		SW	1	Cloudy.
	60	2	0	59	58	29,99	88		SW	1	Cloudy.
18	50	7	0	53	57	29,57	83		S	2	Cloudy.
	56	2	0	55	59	29,57	70		W	2	Fair.
19	39	7	0	40	56	29,62	79		SW	1	Fair.
	50	2	0	49	58	29,63	74		SW	1	Fine.
20	42	7	0	43	57	29,37	89	0,385	NE	1	Cloudy.
	46	2	0	45	57	29,30	83		NE	1	Cloudy.
21	36	7	0	36	56	29,38	83		SW	1	Fine.
	48	2	0	48	57	29,40	71		SW	1	Fine.
22	35	7	0	36	56	29,48	82		SSW	1	Cloudy.
	48	2	0	48	57	29,51	80		SSE	2	Cloudy.
23	42	7	0	44	55	29,53	86	0,145	E	1	Rain.
	45	2	0	44	55	29,54	87		ESE	1	Rain.
24	42	7	0	44	54	29,45	90	0,923	E	1	Cloudy.
	51	2	0	51	56	29,33	83		E	1	Cloudy.
25	42	7	0	42	55	29,12	87	0,050	SE	1	Rain.
	48	2	0	48	56	29,05	80		WSW	1	Cloudy.
26	38	7	0	39	55	29,10	86	0,240	N	1	Cloudy.
	46	2	0	45	56	29,38	83		W	1	Cloudy.
27	35	7	0	37	54	29,76	87		N	1	Fair.
	46	2	0	46	55	29,80	83		NE	1	Cloudy.
28	35	7	0	35	54	29,96	85		NE	1	Fair.
	47	2	0	47	54	29,94	79		NE	2	Cloudy.
29	36	7	0	42	53	29,73	81		NE	2	Cloudy.
	42	2	0	42	54	29,57	85		NE	1	Rain.
30	41	7	0	42	53	29,60	85	0,095	NE	2	Cloudy.
	46	2	0	46	54	29,63	83		NE	2	Cloudy.
31	41	7	0	42	53	29,77	81		NE	2	Cloudy.
	46	2	0	46	54	29,90	75		NE	2	Cloudy.

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for November, 1797.

1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
	°	H.	M.	°	°	Inches.		Inches.	Points.	Str.	
Nov. 1	34	7	0	35	52	30,18	81		NE	2	Fine.
	47	2	0	47	54	30,27	73		NE	2	Fair.
2	31	7	0	32	52	30,23	82		W	1	Cloudy.
	44	2	0	44	53	30,09	85		SW	1	Cloudy.
3	42	7	0	44	53	29,89	88		SW	2	Cloudy.
	52	2	0	52	55	29,84	81		SW	1	Cloudy.
4	44	7	0	44	54	29,70	83		SW	1	Cloudy.
	48	2	0	48	56	29,57	83		E	1	Cloudy.
5	44	7	0	45	54	29,41	87	0,102	E	1	Rain.
	55	2	0	55	58	29,43	89		S	2	Cloudy.
6	50	7	0	50	55	29,88	89		SE	1	Fair.
	57	2	0	57	59	29,96	86		E	1	Fine.
7	49	7	0	48	57	30,11	91		E	1	Foggy.
	55	2	0	55	59	30,17	89		E	1	Fair.
8	47	7	0	47	58	30,39	91		E	1	Foggy.
	54	2	0	53	59	30,41	89		E	1	Fair.
9	41	7	0	42	57	30,48	90		E	1	Foggy.
	48	2	0	47	58	30,46	89		E	1	Cloudy.
10	44	7	0	44	57	30,39	88		E	1	Cloudy.
	52	2	0	50	57	30,39	82		E	1	Fair.
11	40	7	0	40	56	30,38	88		E	1	Fine.
	52	2	0	52	59	30,35	80		E	1	Fair.
12	40	7	0	40	56	30,24	88				Foggy.
	45	2	0	45	57	30,20	88		W	1	Foggy.
13	34	7	0	34	56	30,11	86		NE	1	Foggy.
	51	2	0	51	58	30,07	87		NE	1	Fair.
14	45	7	0	50	57	30,04	90		S	2	Cloudy.
	53	2	0	53	58	30,11	81		SW	2	Cloudy.
15	43	7	0	44	57	30,09	88	0,102	SW	1	Fine.
	51	2	0	51	59	30,15	78		WSW	1	Fair.
16	41	7	0	42	57	30,15	84		WSW	1	Cloudy.
	52	2	0	52	58	30,16	83		NE	1	Cloudy.

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for November, 1797.

1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°										
Nov. 17	35	7	0	35	57	30,32	85		NE	1	Fair.
	44	2	0	44	57	30,28	79		NE	1	Fine.
18	33	7	0	37	56	29,86	84	0,060	SW	1	Rain.
	46	2	0	44	57	29,76	84		NE	1	Cloudy.
19	31	7	0	32	53	29,88	84		NE	2	Snow.
	37	2	0	37	55	29,96	81		NE	2	Fair.
20	27	7	0	30	53	29,97	83		W	1	Cloudy.
	43	2	0	38	54	29,77	85		SSW	1	Rain.
21	34	7	0	35	52	29,73	83	0,060	NW	1	Fine.
	42	2	0	40	53	29,71	79		NW	1	Fine.
22	38	7	0	39	52	29,22	80		WNW	2	Cloudy.
	45	2	0	45	53	29,14	73		WNW	2	Fair.
23	30	7	0	32	51	29,43	86		SW	1	Cloudy.
	36	2	0	36	51	29,47	84		SSW	1	Cloudy.
24	27	7	0	27	49	29,64	85		SSW	1	Fair.
	36	2	0	36	52	29,68	80		N	1	Cloudy.
25	28	7	0	30	50	29,78	82		NE	1	Cloudy.
	45	2	0	39	52	29,78	80		E	1	Cloudy.
26	44	7	0	53	52	29,02	91	0,380	S	2	Cloudy.
	55	2	0	55	54	29,66	90		S	2	Cloudy.
27	49	7	0	50	54	29,74	88	0,102	SW	2	Fair.
	52	2	0	52	56	29,83	81		SW	1	Hazy.
28	47	7	0	47	54	29,82	90	0,020	SW	1	Rain.
	47	2	0	44	56	29,78	91		NE	1	Rain.
29	35	7	0	35	53	29,78	88	0,502	NE	1	Rain.
	48	2	0	37	53	29,68	87		NE	1	Cloudy.
30	36	7	0	40	52	29,44	90	0,145	SW	1	Cloudy.
	49	2	0	49	56	29,46	90		SW	2	Cloudy.

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for December, 1797.

1797	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hy-gro-me-ter.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.		Inches.	Points.	Str.	
	°										
Dec. 1	45	8	0	48	54	29,35	91	0,058	S	2	Cloudy.
	51	2	0	48	57	29,27	88		S	2	Rain.
2	41	8	0	41	53	29,20	80		S	2	Fine.
	47	2	0	47	55	29,23	78		SW	2	Fair.
3	31	8	0	31	53	29,60	83		WSW	1	Fair.
	38	2	0	38	54	29,78	78		NW	1	Cloudy.
4	29	8	0	30	51	30,02	84		NE	1	Fair.
	52	2	0	42	53	29,85	84		E	1	Cloudy.
5	42	8	0	45	53	29,87	88		WSW	1	Fair.
	54	2	0	54	55	30,01	83		SW	1	Fair.
6	50	8	0	50	55	29,65	88	0,315	W	1	Rain.
	48	2	0	45	57	29,85	70		WNW	1	Fine.
7	33	8	0	35	53	30,16	82		WNW	1	Fair.
	41	2	0	41	57	30,15	76		WNW	1	Cloudy.
8	34	8	0	36	53	30,02	78		NW	1	Cloudy.
	50	2	0	45	54	29,79	80		SW	2	Cloudy.
9	37	8	0	37	51	29,62	84	0,110	SW	1	Fair.
	43	2	0	42	56	29,67	80		S	1	Fine.
10	36	8	0	38	53	29,34	86	0,020	SSW	1	Rain.
	40	2	0	40	55	29,33	77		WSW	1	Fair.
11	31	8	0	33	51	29,53	85		WNW	1	Cloudy.
	34	2	0	34	53	29,65	85		NW	1	Fair.
12	31	8	0	40	50	29,36	84		S	2	Rain.
	43	2	0	43	52	29,11	89		S	2	Rain.
13	34	8	0	35	49	29,33	88	0,075	S	1	Fine.
	44	2	0	43	53	29,28	88		S	2	Rain.
14	38	8	0	38	52	29,07	89	0,625	S	1	Fair.
	44	2	0	44	54	29,12	86		S	1	Fair.
15	37	8	0	38	53	29,50	86		SSW	1	Cloudy.
	51	2	0	47	54	29,44	89		S	2	Rain.
16	48	8	0	49	54	29,50	89	0,105	S	2	Rain.
	53	2	0	53	57	29,48	87		S	2	Cloudy.

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for December, 1797.

1797	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°										
Dec. 17	53	8	0	53	56	29,28	85		S	3	Cloudy.
	50	2	0	48	58	29,44	82		SSW	2	Rain.
18	44	8	0	46	56	29,88	88	0,020	S	2	Cloudy.
	52	2	0	52	58	29,84	85		S	2	Cloudy.
19	50	8	0	52	56	29,98	83		SE	2	Cloudy.
	56	2	0	56	58	30,02	83		SSE	2	Fair.
20	51	8	0	51	57	30,17	88		SW	1	Cloudy.
	54	2	0	54	60	30,22	87		W	1	Cloudy.
21	34	8	0	36	57	30,32	85		NE	1	Foggy.
	45	2	0	44	57	30,24	84		E	1	Cloudy.
22	43	8	0	44	57	30,04	89	0,210	SE	1	Rain.
	49	2	0	49	59	29,95	89		SE	1	Cloudy.
23	40	8	0	40	54	29,87	89				Foggy.
	40	2	0	39	58	29,88	88		NE	1	Cloudy.
24	35	8	0	35	56	30,18	88		SW	1	Cloudy.
	37	2	0	37	57	30,24	88		WNW	1	Cloudy.
25	32	8	0	32	54	30,45	87		W	1	Fair.
	38	2	0	38	57	30,45	86		WSW	1	Fair.
26	33	8	0	47	55	30,34	91	0,068	SW	1	Cloudy.
	50	2	0	50	57	30,33	91		SW	1	Cloudy.
27	47	8	0	47	55	30,32	89		SW	1	Cloudy.
	50	2	0	50	58	30,32	87		WSW	1	Cloudy.
28	31	8	0	33	54	30,46	86		SW	1	Fair.
	42	2	0	41	56	30,43	81		WNW	1	Fair.
29	40	8	0	42	54	30,20	85		W	1	Cloudy.
	47	2	0	46	56	29,95	84		W	1	Cloudy.
30	41	8	0	41	53	29,80	76	0,005	W	2	Fair.
	46	2	0	44	56	29,93	75		NW	1	Fair.
31	38	8	0	44	54	29,63	78		WNW	1	Cloudy.
	47	2	0	46	55	29,62	76		W	1	Cloudy.

1797.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	49	25	37,3	49	25	37,7	56	45	51,2	30,50	29,52	30,09	90	69	85,1	0,960
February	50	24,5	37,5	50	25	37,9	57	50	53,3	30,62	29,37	30,31	88	67	81,1	0,219
March	54	27,5	39,9	54	29	40,2	59	51	54,3	30,42	29,44	29,94	86	60	76,6	0,777
April	65	34	47,4	65	35	47,8	62	55	57,8	30,13	29,10	29,77	87	63	77,3	1,859
May	79	34	53,8	78	40	55,4	68	56	61,5	30,33	29,38	29,89	90	61	75,1	1,436
June	73	40	57,5	73	45	58,6	65	59	61,8	30,29	29,36	29,86	85	64	74,3	4,223
July	85	48	65,8	84	55	66,7	74	62	67,4	30,25	29,51	29,96	83	64	74,6	1,288
August	76	48	61,9	76	52	62,6	69	63	66,1	30,18	29,48	29,87	88	66	76,4	2,789
September	71	42	56,9	69	45	57,5	67	60	62,2	30,14	29,04	29,75	90	65	79,3	4,061
October	63	35	49,0	62	35	49,6	62	53	57,5	30,31	29,05	29,83	90	67	81,3	2,001
November	57	27	43,3	57	27	43,4	59	49	55,0	30,48	29,14	29,92	91	73	85,0	1,473
December	56	29	42,7	56	30	43,0	60	49	54,9	30,46	29,07	29,80	91	70	84,5	1,611
Whole year			49,4			50,0			58,6			29,92			79,2	22,697

* The quicksilver in the bason of the barometer is 81 feet above the level of low water spring tides at Somerset-house.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCXCVIII.

PART II.

LONDON,

SOLD BY PETER ELMSLY,
PRINTER TO THE ROYAL SOCIETY.

MDCCXCVIII.

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ERRATA

In Mr. ATWOOD's *Disquisition on the Stability of Ships.*

Page 213, lines 8 and 9, *dele* "situated between wind and water, according to a technical expression."

Page 223, l. 22, *for* $+$ SB, *read* $+ \frac{SB}{3}$.

—— 247, l. 9, *for* $\frac{\text{Flu. } \dot{x} \times \overline{y^3 + p^3}}{3p}$, *read* fluent of $\dot{x} \times \frac{\overline{y^3 + v^3}}{p}$.

—— 252, l. 25, *after* point V, *add* produce XL *in directum,* and in the line so produced.

—— 254, l. 22, after the words "proportion of 3 to 5," instead of the *comma* insert a *colon*.

Page 268, l. 8, *for* DD′G′GA, *read* DD′G′GD.

PHILOSOPHICAL

TRANSACTIONS.

X. *A Disquisition on the Stability of Ships. By* George Atwood, *Esq. F. R. S.*

Read March 8, 1798.

THE stability of vessels, by which they are enabled to carry a sufficient quantity of sail, without danger or inconvenience, is reckoned amongst their most essential properties; although the wind may, in one sense, be said to constitute the power by which ships are moved forward in the sea, yet, if it acts on a vessel deficient in stability, the effect will be to incline the ship from the upright, rather than to propel it forward: stability is therefore not less necessary than the impulses of the wind are, to the progressive motion of vessels. This power has also considerable influence in regulating the alternate oscillations of a ship in rolling and pitching; which will be smooth and equable, or sudden and irregular, in a great measure, according as the stability is greater or less at the several angles of inclination from the upright. From constantly observing that the performance of vessels at sea depends materially on their stability, both navigators and naval architects must, at all

times, be desirous of discovering in what particular circumstances of construction this property consists, and according to what laws the stability is affected by any varieties that may be given to their forms, dimensions, and disposition of contents; which are determined partly according to the skill and judgment of the constructor, and partly by adjustments after the vessel has been set afloat.

Little more than a century has now elapsed, since the theory of mechanics was first applied to the construction and management of vessels; whatever principles had been previously adopted, for regulating their forms and equipment, as well as for directing them in the ocean, were the result of experience and observation alone: a mode of arriving at truth, however advantageous in many respects, yet not entirely to be relied on in this instance, for explaining satisfactorily, and reducing to system, phenomena depending on the intricate combination of causes which influence a vessel's motion, and equilibrium, at sea. The theory of mechanics is known to explain all effects that can arise from the action of forces, however complicated, of which the quantities and directions are defined with sufficient precision. This science, having been greatly extended, and successfully employed, by Sir ISAAC NEWTON, in the investigation of causes requiring the most profound research, would naturally be resorted to, for a solution of many difficulties that occur in the theory of naval architecture, which could not be obtained from any other mode of considering this subject. The practice of ship building having been many ages antecedent to the discovery of the theory of mechanics, one object of theoretic inquiry must necessarily be, to explain the principles of construction and management which experience and practical

observation have previously discovered; distinguishing those which are founded in truth and right practice, from others which have been the offspring of vague and capricious opinion, misinterpretation of facts, and unfounded conjecture, by which, phenomena arising in the practice of navigation are often attributed to causes entirely different from those by which they are really governed. It is also the object of mechanic theory to investigate, from the consideration of any untried plans of construction, what will be the effect thereof on the motion of vessels at sea; also to suggest new combinations, by which the approved qualities of vessels may be extended, their faults amended, or defects supplied. These several objects, and others connected with them, have employed the attention of many eminent theorists, by whose discoveries naval architecture has been greatly benefited; yet the progress made toward establishing a general theory, founded on the laws of motion, has not been adequate to what might be expected from the abilities of the writers on this subject, and the laborious attention they have bestowed upon it. Although all results deduced by strict geometrical inference from the laws of motion, are found, by actual experience, to be perfectly consistent with matter of fact, when subjected to the most decisive trials, yet, in the application of these laws to the subject in question, difficulties often occur, either from the obscure nature of the conditions, or the intricate analytical operations arising from them, which either render it impracticable to obtain a solution, or, if a result is obtained, it is expressed in terms so involved and complicated, as to become in a manner useless, as to any practical purpose. These imperfections in the theory of vessels, are amongst the causes which have contributed to

retard the progress of naval architecture, by increasing the hazard of failure in attempting to supply its defects by experiment; for, when no satisfactory estimate can be formed from theory, of the effects likely to ensue from adopting any alteration of construction that may be proposed, doubts must necessarily arise respecting its success or failure, which can be resolved only by having recourse to actual trial: a species of experiment rarely undertaken under the impressions of uncertain success, when the objects of it are so costly, and otherwise of so much importance. To the imperfections of theory, may also be attributed that steady adherence to practical methods, rendered familiar by usage, which creates a disposition to reject, rather than to encourage, proposals of innovation in the construction of vessels: the defects or inconveniences which are known, and have become easily tolerable by use, or may perhaps be the less distinctly perceived for want of comparison with more perfect works of art, being deemed preferable to the adoption of projected improvements, attended by the danger of introducing evils, the nature and extent of which cannot be fully known. These are amongst the difficulties and disadvantages which have concurred in rendering the progress of improvement, in the art of constructing vessels, extremely slow, and have left many imperfections in this practical branch of science, which still remain to be remedied. In respect to the theory of vessels, it would be giving that term too narrowed a meaning, to consider it as derived solely from the laws of mechanics; every notion or opinion which may be applied to explain satisfactorily the phenomena depending on a vessel's construction and qualities, so as to infer the consequences of given conditions, independently of actual trial, whether it ori-

ginates from experience alone, or from investigations founded on the laws of motion, is to be regarded as forming a part of this theory, in which, a constant reference to practice is so essentially necessary. For, although many principles are deducible from the laws of mechanics, which it is probable that no species of experiment, or series of observation, however long continued, would discover, yet there are others, no less important, which have been practically determined with sufficient exactness, the investigation of which it is scarcely possible to infer from the laws of motion; the complicated and ill defined nature of the conditions, in particular instances, rendering analytical operations founded on them liable to uncertainty. Since the practice of naval architecture depends so materially on the knowledge of the causes which influence the motion of vessels at sea, much benefit may probably be derived from the extension of well founded principles, both by attentive observation of the qualities of vessels, compared with their construction, as well as by investigation of the effects arising from particular modes of construction, depending on the laws of statics and mechanics, whenever the conditions admit of inferring principles which are clear and satisfactory, and easily applicable in practice. With a view to these objects, so far as regards the theory of stability, the ensuing Disquisition has been written.

When a ship, or other floating body, is deflected from its quiescent position, the force of the fluid's pressure operates to restore the floating body to the situation from which it has been inclined. This force is distinctly described, in a treatise written by the most celebrated geometrician of ancient times, who uses the following argument for demonstrating the position in which a parabolic conoid will float permanently in given cir-

cumstances. To shew that this solid will float with the axis inclined to the fluid's surface at a certain stated angle, depending on the specific gravity and dimensions of the solid, he demonstrates,* that if the angle should be greater than that which he has assigned, the fluid's pressure will diminish it; and that, if the angle should be less, the fluid's pressure will operate to increase it, by causing the solid to revolve round an axis which is parallel to the horizon. It is an evident consequence, that the solid cannot float quiescent with the axis inclined to the fluid's surface, at any angle except that which is stated. The force which is shewn in this proposition, to turn the solid, so as to alter the inclination of the axis to the horizon, is the same with the force of stability; the quantity or measure of which, Archimedes does not estimate; nor was it necessary to his purpose, since the alteration of inclination required to establish the quiescent position, may be produced either in a greater or less time, without affecting his argument. It does not appear, that this method of determining the floating positions of bodies was afterwards extended to infer similar conclusions in respect to solids of any other forms, nor to determine any thing concerning the inclination or equilibrium of ships at sea, which require the demonstration, not only that a force exists, in given circumstances, to turn the vessel round an axis, but also the magnitude or precise measure of that force. M. Bouguer, in his treatise intitled "*Traitè du Navire,*" † has investigated a theorem for estimating the exact measure of the stability of floating bodies. This theorem, in one sense, is general, not being confined to bodies of any particular form; but, in respect to the angles of

* Archimedes *de iis quæ in humido vehuntur.* † *Livr.* ii. *sect.* 2. *chap.* 8.

inclination, it is restrained to the condition that the inclinations from the upright shall be evanescent, or, in a practical sense, very small angles. In consequence of this restriction, the rule in question cannot be generally applied to ascertain the stability of ships at sea; because the angles to which they are inclined, both by rolling and pitching, being of considerable magnitude, the stability will depend, not only on the conditions which enter into M. BOUGUER's solution, but also on the shape given to the sides of the vessel above and beneath the water-line or section, of which M. BOUGUER's theorem takes no account. But it is certain that the quantity of sail a ship is enabled safely to carry, and the use of the guns in rough weather, depend in a material degree on the form of the sides above and beneath the water-line; this observation referring to that portion of the sides only which may be immersed under, or may emerge above, the water's surface, in consequence of the vessel's inclination; for, whatever portion of the sides is not included within these limits, will have no effect on the vessel's stability, the centres of gravity, volume of water displaced, and other elements not being altered. By the water-section is meant, the plane in which the water's surface intersects the vessel, when floating upright and quiescent; and the termination of this section in the sides of the vessel is termed the water-line. A general theorem for determining the floating positions of bodies is demonstrated in a former paper, inserted in the Phil. Trans. for the year 1796, and applied to bodies of various forms: the same theorem is there shewn to be no less applicable to the stability of vessels, taking into account the shape of the sides, the inclination from the upright, as well as every other circumstance by which the

stability can be influenced. To infer, from this theorem, the stability of vessels in particular cases, the form of the sides, and the angle of inclination from the perpendicular, must be given. These conditions admit of great variety, considering the shape of the sides, both above the water-line and beneath it; for we may first assume a case, which is one of the most simple and obvious; this is, when the sides of a vessel are parallel to the plane of the masts, both above and beneath the water-line; or, secondly, the sides may be parallel to the masts under the water-line, and project outward, or may be inclined inward, above the said line; or they may be parallel to the masts above the water-line, and inclined either inward or outward beneath it; some of these cases, as well as those which follow, being not improper in the construction of particular species of vessels, and the others, although not suited to practice, will contribute to illustrate the general theory. The sides of a vessel may also coincide with the sides of a wedge, inclined to each other at a given angle; which angle, formed at an imaginary line, where the sides, if produced, would intersect each other, may be situated either under or above the water's surface. To these cases may be added, the circular form of the sides, and that of the Apollonian or conic parabola. The sides of vessels may also be assumed to coincide with curves of different species and dimensions, some of which approach to the forms adopted in the practice of naval architecture, particularly in the larger ships of burden. And lastly, the shape of the sides may be reducible to no regular geometrical law; in which case, the determination of the stability, in respect to a ship's rolling, requires the mensuration of the ordinates of the vertical sections which intersect the longer

axis at right angles; similar mensurations are also required for determining the stability, in respect to the shorter axis, round which a vessel revolves in pitching. In order to describe distinctly these several cases, the variation of the sections, both in form and magnitude, from head to stern of the vessel, has not been considered; the sections being supposed equal and similar figures, such as they in reality are, near the greatest section of a ship, growing smaller, and altering their form, toward the head and stern. But, before this alteration can be taken into account, it is necessary first to ascertain the stability corresponding to a vessel or segment, in which the sections are equal and similar figures; from which determination, the stability is inferred which actually exists, when the form and magnitude of the sections alter continually, from one extremity of the vessel to the other. The consideration of the cases which have been here stated, with inferences and observations thereon, is the subject of the ensuing pages; in which, if any ideas are suggested which may be at all useful in the practice of naval architecture, or may contribute to remove imperfect or erroneous notions which have been entertained respecting a principal branch of it, the intention of the Author will be accomplished.

Let WBCOFAH (Tab. VIII. fig. 1.) represent a vertical section of a vessel floating quiescent and upright, and intersected by the water's surface in the line BA: BCOFA will be the area immersed under water. Suppose the vessel to be inclined from the perpendicular, through the angle ASH, so that the intersection of the vessel by the water's surface, which before coincided with BA, shall now coincide with the line

CH: the area under water will now be COFAH, equal to the area BCOFA.

Let the section WBCOFAH, and all the other vertical sections intersecting the longer axis at right angles, be assumed similar and equal figures, projected on the plane WBOAH: in consequence, the area BOA will be to the area ASH, as the entire volume immersed is to the volume immersed by the vessel's inclination. Moreover, if E is the centre of gravity of the area BOA, that point will truly represent the centre of gravity of the volume immersed, when the vessel is upright: if the centre of gravity of the immersed area COFAH, when the vessel is inclined, should be situated at Q, that point will also coincide with the centre of gravity of the corresponding displaced volume. For these reasons, the spaces BOA, ASH, COFAH, will be denominated, in the following pages, indifferently, areas or volumes.

Let G be the centre of gravity of the vessel, by which term, the vessel and its contents, of every kind, are always understood to be implied. Through G, draw GU parallel to CH: and through Q, draw QZ perpendicular to CH. When the ship is inclined round the longer axis, through the angle ASH, the fluid's pressure acts in the direction of the vertical line QZ, with a force equal to the vessel's weight; and the stability or effect of this force, to turn the vessel round an axis passing through G, perpendicular to the plane BOA, will be greater or less, according to the magnitude of the line GZ, or distance from the axis at which the force of pressure acts. In the same vessel, the weight not being altered, the stability, at different angles of inclination from the upright, will be truly measured by the line GZ; and, in different vessels, or in the

same vessel differently laden, the stability will be measured by the weight of the vessel and the line G Z jointly. The weight of any vessel (including the lading) is equal to the weight of water displaced by it; which will be obtained by measuring the solid contents of the displaced volume, and from knowing the weight of a given portion of sea water, such as a cubic foot, which weighs 64 pounds avoirdupois. The vessel's weight being thus obtained, the determination of the stability, whatever be its form or inclination from the upright, requires only that the line G Z shall be known, or the proportion which it bears to some given line, for instance, the line B A, shall be ascertained.

A general method of constructing this line is demonstrated in the Phil. Trans. for the year 1796, but is there principally applied to the floating position of bodies; its use in investigating the stability of vessels is incidentally mentioned, and in general terms, rather than as being itself a subject of disquisition. This theorem is founded on supposing the centres of gravity of the several volumes BOA, COFH, ASH, BSC, (fig. 1.) to be given in position; an assumption allowable in demonstrating a general theorem: but, in applying it to the stability of particular vessels, it becomes necessary that the positions of these points should be absolutely found, and the results combined with the other conditions, to infer the measure of stability; a determination which, in some cases, is attended with much difficulty, and in others, is not practicable by any direct methods; an instance, amongst many that might be mentioned, in which the particular application is more difficult than the general demonstration of propositions. The following constructions and investigations are

principally inferred from the general theorem for ascertaining the stability of floating bodies; which is here subjoined, to avoid the necessity of future references, as well as for the purpose of stating more distinctly the observations which follow it.

Let M (fig. 1.) be the centre of gravity of the volume ASH, which has been immersed under water, and let I be the centre of gravity of the volume BSC, which has emerged above the water's surface, in consequence of the vessel's inclination; through the points M and I, draw the lines ML, IK, perpendicular to the line CH, which coincides with the water's surface when the vessel is inclined: through E, the centre of gravity of the displaced volume BOA, draw EV parallel and equal to KL, and through G draw GU parallel and GR perpendicular to CH; according to the theorem, the line ET will be determined by the following proportion. As the total volume displaced BOA is to SAH, the volume immersed in consequence of the inclination, so is KL or EV to ET; and, since the angle EGR is equal to the vessel's inclination ASH, and the distance GE is supposed to be given, the line ER will be known; because ER is to GE as the sine of the angle EGR to radius; ER being subtracted from ET will leave RT or GZ, equal to the measure of the vessel's stability.

Suppose the line KL to be denoted by the letter b: let the volume ASH be represented by A, and the volume BOA by V. Then, according to the theorem, since $V : A :: : : b : ET$, it follows that $ET = \frac{bA}{V}$, and if GE is put $= d$, and s $=$ the sine of the angle to which the vessel is inclined, radius being $= 1$, ER will be $= ds$; and the measure of the vessel's stability RT or $GZ = \frac{bA}{V} - ds$.

Through the points C and H, (fig. 1.) let the lines CF, WH, be drawn parallel to BA. The position of the points M and I, the magnitude of the line KL, and the areas or volumes ASH, BSC, being the same, whatever alteration may take place in the volume V, or the entire volume displaced, the quantity KL × area ASH or bA will remain the same: and, since the line $ET = \frac{bA}{V}$, it will follow, that the zone WHFC, situated between wind and water, (according to a technical expression,) not being altered, ET will be in the inverse proportion of V, or the total volume displaced. If, therefore, the shape of the vessel under the line CF should be any how changed, so as to coincide with another figure, suppose C*cf*F, (fig. 2.) instead of COF, (fig. 1.) the volume C*cf*F being equal to the volume COF, the line ET will be the same in both cases. In consequence of this change of figure, the position of the point E, (fig. 1.) or centre of gravity of the volume BOA, may be situated higher or lower in the line OD; yet, if the centre of gravity G is so adjusted by ballast, or other means, that the distance GE shall be the same, the stability of each vessel, BCOA (fig. 1.) and BC*cf*A (fig. 2.) will be perfectly the same, when inclined to the same angle ASH from the upright. It must also be observed, that since ET is always greater in the same proportion in which the volume immersed BOA is less, the zone WHCF being both in magnitude and form the same, having found by construction or calculation the value of the line ET corresponding to any given volume displaced, suppose V = BCOA, (fig. 1.) the line E*t* corresponding to any other magnitude of volume displaced, suppose v = BCV*wl*FA, (fig. 2.) will be immediately inferred; for, since V . v : : E*t* : ET, it

follows that $Et = \frac{ET \times V}{v}$, or because $ET = \frac{bA}{V}$, by substitution, $Et = \frac{bA}{v}$. For these reasons, the determination of stability does not require that the form of the entire volume displaced should be given, but the form only of the zone WCHF, (fig. 1. and 2.) including the angle of the vessel's inclination ASH; these conditions, together with the magnitude of the immersed volume, and the distance between the two centres of gravity G and E, are sufficient for finding the measure of stability, at any given angle of inclination from the upright.

CASE I.

The sides of a vessel are parallel to the plane of the masts, both above and beneath the water-line.

QBCOAH (fig. 3) coincides with the vertical section of a vessel when it floats upright and quiescent, and is intersected by the water's surface in the line BA; the sides QC, HD, are parallel to each other, and to the plane of the masts WO, and are therefore perpendicular to BA. G is the centre of gravity of the vessel; V represents the magnitude of the volume immersed under the water; the centre of gravity of this volume is situated at E. Suppose the vessel to be inclined from its quiescent position through any given angle, it is required to express, by geometrical construction, the measure of the vessel's stability, when thus inclined. Bisect BA in the point S, and through S draw CSH, inclined to BA, at the given angle of the vessel's inclination from the upright. Bisect BC in F, and AH in N; and join SF and SN. In the line SF take SI to SF as 2 to 3; also, in the line SN, take

SM to SN as 2 to 3. Through the points I and M, draw IK, ML, perpendicular to CH. Through the point E, draw EV parallel and equal to KL. In the line EV, take ET to EV, in the proportion which the volume ASH bears to the entire volume displaced. Through G, draw GU parallel to CH; and through T, draw TZ perpendicular to GU. GZ is the measure of the vessel's stability. The demonstration of this construction evidently follows from the general theorem.

From this construction, the value of GZ, or measure of the vessel's stability, may be investigated analytically, and expressed in general terms. Through G, draw GR perpendicular to EV. Let $BA = t$, $GE = d$, the angle $ASH = S$; radius $= 1$. The rules of trigonometry give the following determinations. $AN = \frac{t \times \text{tang.}\,S}{4}$: $SN = \frac{t}{4} \times \sqrt{4 + \text{tang.}^2 S}$. Also, as $SN : HN :: \text{sine } NHS : \text{sin. } NSH$, or $\frac{t}{4} \times \sqrt{4 + \text{tang.}^2 S} : \frac{t \times \text{tang.}\,S}{4} :: \text{cos.}\,S : \text{sin.}\,NSH$. Wherefore $\text{sin.}\,NSH = \frac{\text{sin.}\,S}{\sqrt{4 + \text{tang.}^2 S}}$; $\text{cos.}^2 NSH = \frac{4 + \text{tang.}^2 S - \text{sin.}^2 S}{4 + \text{tang.}^2 S} = \frac{2 + \text{sec.}^2 S + \text{cos.}^2 S}{4 + \text{tang.}^2 S} = \frac{\overline{\text{sec.}\,S + \text{cos.}\,S}|^2}{4 + \text{tang.}^2 S}$ (because $2 \times \text{cos.}\,S \times \text{sec.}\,S = 2$) consequently $\text{cos.}\,NSH = \frac{\text{sec.}\,S + \text{cos.}\,S}{\sqrt{4 + \text{tang.}^2 S}}$. And since by construction, $SM = \frac{2}{3} SN$, and $SN = \frac{t}{4} \times \sqrt{4 + \text{tang.}^2 S}$, $SM = \frac{t}{6} \times \sqrt{4 + \text{tang.}^2 S}$, and $SL = \frac{t}{6} \times \sqrt{4 + \text{tang.}^2 S} \times \frac{\text{sec.}\,S + \text{cos.}\,S}{\sqrt{4 + \text{tang.}^2 S}} = \frac{t}{6} \times \overline{\text{sec.}\,S + \text{cos.}\,S}$: and the triangles SLM, SIK being similar and equal, $KL = 2\,SL$: Wherefore $KL = \frac{t}{3} \times \overline{\text{sec.}\,S + \text{cos.}\,S} = EV$. The area of the triangle $ASH = \frac{t^2 \times \text{tang.}\,S}{8}$ representing the volume immersed by the vessel's inclination; and by construction,

As V : volume A S H : : E V : E T, or

$V : \frac{t^2 \times \text{tang.}\, S}{8} : : \frac{t}{3} \times \overline{\text{sec.}\, S + \text{cos.}\, S} : ET$; this will give the value of $ET = \frac{t^3 \times \text{tang.}\, S \times \overline{\text{cos.}\, S + \text{sec.}\, S}}{24\, V}$: and because

$ER : EG : : \text{sin.}\, S : 1$, and $EG = d$, it follows, that $ER = d \times \text{sin.}\, S$; and therefore R T, or the measure of the vessel's stability $GZ = \frac{t^3 \times \text{tang.}\, S}{24\, V} \times \overline{\text{cos.}\, S + \text{sec.}\, S} - d \times \text{sin.}\, S$.

To exemplify this determination by referring to a particular case, let the vessel's breadth at the water's surface, or BA, be divided into 100 equal parts, and let G E be 13 thereof; so that $t = 100$, and $d = 13$. Suppose the inclination of the vessel from the perpendicular, or A S H, to be $15°, = S$; and let the area B C O D A, representing the volume displaced, be equal to a square of which the side is = 60; so that the area V shall = 3600: then, referring to the solution, we obtain

$$\text{cos.}\, S + \text{sec.}\, S = 2.0012$$

$$\text{Also } \frac{t^3 \times \text{tang.}\, S}{24\, V} = \frac{1000000 \text{ tang } 15°}{24 \times 3000} = 3.1013$$

$$ET = 2.0012 \times 3.10190 \qquad = 6.2063$$

$$d \times \text{sin.}\, S = 13 \times \text{sin}\, .15° \qquad = 3.3646$$

$$\text{measure of stability, or } GZ \qquad = 2.8417$$

It appears by this result, that when the vessel has been inclined from the upright through an angle of 15°, the direction of the fluid's pressure, acting to restore the quiescent position, will pass at a distance estimated horizontally from the axis = 2.84, when the breadth B A = 100. And this will be true, whatever be the length of the axis.

The fluid's pressure is the weight of water displaced, the magnitude of which depends both on the area of the vertical

sections, and length of the axis: suppose this weight to be 1000 tons; according to the preceding determination, the stability of the vessel, when inclined from the upright to an angle of 15°, will be a pressure equal to the weight of 1000 tons, acting at a distance of $\frac{2,84}{100}$ parts of the breadth BA from the axis, to restore the vessel to the position from which it has been inclined. This force is the same as if a pressure of $\frac{1000 \times 2,84}{50} = 56.8$ tons, should be applied to turn the vessel at the distance of 50 from the axis: if therefore the wind, or other equivalent power, should act on the sails of the vessel with a force of 56.8 tons, at the mean or average distance of 50, or $\frac{1}{2}$ the breadth BA from the axis, to incline the ship, the force of stability will just balance it, so as to preserve an equilibrium; the vessel continuing inclined from the upright at the angle of 15°. If the wind's force should be less, the inclination must necessarily be diminished; if greater, it must be increased, until the two forces balance each other. Here it is to be observed, that the force of the wind is estimated in a direction which is perpendicular to the plane of the masts.*

* In this and the following numerical examples, in order to bring into comparison the effect of giving different forms to the sides of vessels, their weights, and all the other conditions (the figure of the sides excepted) on which the stability depends, are assumed to be the same. The measures of stability are compared, both by the relative distances from the axis at which a given pressure, equal to the vessel's weight, acts to turn the ship round the longer axis, and by the relative equivalent weights which act at a given distance from the axis. By the latter method, the proportions of stability are perhaps more distinctly expressed than by the former, although both are essentially the same.

The mechanical force employed to incline a vessel from the upright, through any given angle, for the purpose of examining and repairing the bottom of a ship, is to be ascertained from the theorems here given for expressing the measures of stability,

CASE II.

The sides of a vessel project outward above the water-line, and are parallel to the masts under the water-line.

The line B A (fig. 4.) represents the intersection of the water's surface with the vessel, when floating upright. The lines PC, QW, are parallel to each other, and to the line XO, which coincides with the plane of the masts, and bisects the line BA in the point D; B C and AW, which are parallel to the plane of the masts, coincide with the sides of the vessel under the water-line; and BY, AH, which project outwards from the plane of the masts, at the angle QAH, or YBP, are the sides of the vessel above the water-line. C H represents the intersection of the water's surface with the vessel, when inclined from the perpendicular, through a given angle OPQ = ASH. The distance G E, between the centres of gravity of the vessel and of the volume displaced, and the magnitude of that volume being supposed known, and the angle QAH, at which the sides AH, BY, are inclined to the plane of the masts, being also known, it is required to ascertain, by geometrical construction, the measure of the vessel's stability, when the

which is exactly equal to the force to be applied for that purpose. Another method of inclining a vessel (well adapted for making experiments on this subject) is, by applying a timber at right angles to the plane of the masts. If a weight be affixed to one of its extremities, from having given the weight so applied, and its distance from the plane of the masts, together with the other conditions which determine stability, the angle of inclination, through which the ship will be inclined, may be determined by the theorems in these pages. The same inferences may be obtained, from having given the weights and spaces through which the guns are run out on one side, and drawn in on the other, instead of the weight affixed, according to the method last described.

vessel is inclined from the perpendicular, through an angle equal to the angle OPQ.

At whatever angle the vessel may be inclined from the perpendicular, the total volume immersed must always remain the same, while the vessel's weight continues unaltered. Wherefore, the volume which has been immersed, or ASH, must be equal to the volume BSC, which has emerged from the water, in consequence of the vessel's inclination. For this reason, and because the side AH projects outward, while the side BC is parallel to the plane of the masts, it must necessarily happen, that the point S will not in this case bisect the line BA, as it did in the preceding construction, but will be removed nearer to the side AH, which has been immersed by the inclination of the vessel. Previously, therefore, to any consideration of the stability, it will be necessary to define the position of the point S in the line AB, so that a line CH, being drawn through S, at a given angle of inclination to AB, equal to that of the ship's inclination from the perpendicular, shall cut off the area ASH equal to the area BSC.

Let the given angle of inclination be OPQ equal to ASH (fig. 4.): the angle QAH, at which the sides of the vessel are inclined outward from the plane of the masts above the water-line, is supposed to be given: this angle + 90° will be the angle SAH, which is therefore a known quantity: the remaining angle SHA, in the triangle ASH, will likewise be known.

Through the extremity B of the line BA, (fig. 5.) equal to the vessel's breadth at the water-line, draw the indefinite line BU inclined to BA, at an angle ABU equal to OPQ: in BU, take any point O, and, in the line BO, set off BD to BO, as the cosine of the angle ABU to radius. In the line BD, take BE to BD, as the sine of the angle BAU is to radius: also take BF to

BO, as the sine of the angle AUB to radius. Let BG be taken a geometrical mean proportional between the lines BF and BE; and from the point G, in the direction of the line GU, set off GZ equal to BF: join AZ; and, through the point G, draw GS parallel to ZA, intersecting BA in the point S. Through S, draw the line CH parallel to BU: the area ASH will be equal to the area BSC.

Since, in the triangles ASH, BSC, the angle ASH is equal to the angle BSC, the areas of the triangles will be in a ratio compounded of the ratios of the sides, including the equal angles; that is, the area of the triangle ASH, will be to the area of the triangle BSC in the ratio of $SA \times SH$ to $SB \times SC$. By the construction, the angle ASH = the angle ABU = OPQ; and the angle AHS = the angle AUB: also, by construction,

$$\begin{array}{rl} & BO : BD :: \text{rad.} : \text{cos. } ASH. \\ \text{Also} & BD : BE :: \text{rad.} : \text{sin. } SAH. \\ \text{And} & \underline{BF : BO :: \text{sin. } AHS : \text{rad.}} \\ \text{Joining these ratios} & BF : BE :: \text{sin. } AHS \times \text{rad.} : \text{cos. } ASH \times \text{sin. } SAH. \end{array}$$

But, by the construction, and by the similarity of the triangles,

$$\begin{array}{rl} BGS,\ BZA, & \underline{BF \text{ or } GZ : BE^{*} :: BF^2 : BG^2 :: GZ^2 : BG^2 :: SA^2 : SB^2 :} \\ \text{Wherefore} & SA^2 : SB^2 :: \text{sin. } AHS \times \text{rad.} : \text{cos. } ASH \times \text{sin. } SAH. \\ \text{And by trigonometry} & SH : SA :: \text{sin. } SAH \quad : \text{sin. } AHS; \\ \text{and} & \underline{SB : SC :: \text{cos. } ASH \quad : \text{rad.}} \\ \text{Joining these ratios} & SA \times SH : SB \times SC :: \text{rad.} \quad : \text{rad.} \end{array}$$

But the area ASH is to the area BSC as $SA \times SH$ to $SB \times SC$; consequently, the area ASH is equal to the area BSC.

To proceed with the construction of the second case. Through the point S, (fig. 4.) determined by the preceding construction, draw the line CH inclined to BA at the angle

* Because the ratio of BE to BG is equal to the ratio of BG to BF, by the construction, it will follow that the ratio of BE to BF is double the ratio of BE to BG.

ASH, equal to the given angle OPQ: when the vessel is inclined from the perpendicular through this angle, it will be intersected by the water's surface coinciding with the line C H. Bisect B C in F, and AH in N; and join S F, S N: take S I to S F, as 2 to 3; and S M to S N, in the same proportion. Through I and M, draw the lines IK, ML, perpendicular to C H. Through the centre of gravity of the vessel G, draw G U parallel to C H; and through the centre of gravity E, of the displaced volume B O A, draw E V parallel and equal to K L; and in E V take E T to E V, in the same proportion which the volume A S H bears to the entire volume displaced B OA. Through T, draw T Z perpendicular to G U. G Z is the measure of the vessel's stability.

To obtain an analytical value of the line GZ, for brevity, let the sine of the angle AS H be denoted by s, when radius is $=1$, make sin. H A S $= a$; sin. AH S $= b$; sin. S C B $= c$. Let G E $= d$. Also, let the entire volume displaced $=$ V. By the rules of trigonometry, it is found that

$$* \text{ SL} = \frac{\text{SA}}{3} \times \sqrt{4 + s^2 \times \frac{1-b^2}{b^2} + \frac{4s \times \sqrt{1-a^2}}{b}}$$

$$\text{Also SK} = \frac{\text{SB}}{3} \times \overline{\text{cos. ASH} + \text{sec. ASH}}.$$

$$\text{The area SCB or ASH} = \frac{\text{SB}^2 \times \text{tang. ASH}}{2}$$

Wherefore $\text{ET} = \frac{\text{SA} \times \text{SB}^2 \times \text{tang. ASH}}{6\text{V}} \times \sqrt{4 + s^2 \times \frac{1-b^2}{b^2} + \frac{4s \times \sqrt{1-a^2}}{b}} + \frac{\text{SB}^3 \times \text{tang. ASH}}{6\text{V}} \times \overline{\text{cos. ASH} + \text{sec. ASH}}$. If the breadth BA

* When the angle SAH $= 90$, $1 - a^2 = 0$; and $b =$ cos. S; in which case, if c is put $=$ cos. S, $\text{SL} = \frac{\text{SA}}{3} \times \sqrt{4 + \frac{s^4}{c^2}}$, but $4 + \frac{s^4}{c^2} = 4 + \text{tang.}^2 \text{S} \times \text{sin.}^2 \text{S} = 4 + \text{tang.}^2 \text{S} - \text{tang.}^2 \text{S} \times \text{cos.}^2 \text{S} = 2 + \text{sec.}^2 \text{S} + \text{cos.}^2 \text{S} = \overline{\text{cos. S} + \text{sec. S}}|^2$. Wherefore $\text{SL} = \frac{\text{SA}}{3} \times \overline{\text{cos. S} + \text{sec. S}}$.

be represented by the letter t, it is inferred, from the construction in p. 220, that $SA = \frac{t \times \sqrt{b}}{\sqrt{b} + \sqrt{ac}}$ and $SB = \frac{t \times \sqrt{ac}}{\sqrt{b} + \sqrt{ac}}$. The value of the line ET having been thus determined, if $ER = d \times$ sin. ASH or ds be subtracted from it, the result will be GZ, the measure of the vessel's stability.

Suppose the sides BY, AH, (fig. 4.) to project outward, at an angle of 15° inclination to the parallel sides BC, AW, so as to make the angle SAH = 105°. Let the vessel's inclination from the upright be the angle ASH = 15°; and therefore AHS = 60°, and SCB = 75°. Let the breadth BA or t = 100 equal parts, of which d or GE = 13. Then, by calculating from the analytical values just determined, it is found that KL = SL + SK = 68,017: the area ASH = 347.44, and the entire volume immersed V, being, as in the former case, = 3600, $ET = \frac{68.017 \times 347.44}{3600} = 6.57$. And, since ER or $d \times$ sin. ASH is = 3.36, if the latter value be subtracted from the former, the result will be GZ = 3.21, or the measure of the vessel's stability.

The force of stability, to restore the vessel to the upright position, will be precisely the vessel's weight, or fluid's pressure, acting in the direction of a vertical line, which passes at a distance of 3.20 from the axis, estimated in a horizontal direction. And this force is equivalent to, and will counterbalance, $\frac{3.21}{50}$ parts of the vessel's weight, applied to act in a contrary direction, at the distance of 50 from the said axis. So that, if the vessel's weight should be 1000 tons, the force of stability would balance a weight or force of $\frac{3.21 \times 1000}{50} = 64.2$ tons, applied to act at the distance 50 from the axis.

CASE III.

The sides of a vessel are inclined inward above the water-line, and are parallel to the plane of the masts under the water-line.

AH, BY, (fig. 6.) are the sides of a vessel inclined inward above the water-line BA, at an angle HAQ = YBP from the direction of the sides AW, BC, under the water-line, which are parallel to each other, and to the plane of the masts. Suppose the vessel to be inclined from the upright, through an angle = OPQ. By the construction, (p. 220.) draw the line CH intersecting BA, in a point S, at an angle ASH equal to the given angle OPQ; so that the area ASH shall be equal to the area BSC. When the vessel has been inclined through the given angle OPQ, it will be intersected by the water's surface in the line CH. The construction of the line GZ, or measure of the vessel's stability, is the same as in the preceding case.

Let the sine of ASH = s; sin. SAH = a; sin. SHA = b; sin. SCB = c to rad. = 1. Also let GE = d.

From the rules of trigonometry, it is inferred that

$$\mathrm{KL} = \frac{\mathrm{SA}}{3} \times \sqrt{4 + s^2 \times \frac{1-b^2}{b^2} - \frac{4s \times \sqrt{1-a^2}}{b}}$$

$$+ \mathrm{SB} \times \overline{\cos.\ \mathrm{ASH} + \sec.\ \mathrm{ASH}}.$$

The area SBC or ASH $= \frac{\mathrm{SB}^2 \times \text{tang. ASH}}{2}$.

If, therefore, the total volume immersed is made = V, the value of the line ET will be

$$\mathrm{ET} = \frac{\mathrm{SA} \times \mathrm{SB}^2 \times \text{tang. ASH}}{6\mathrm{V}} \times \sqrt{4 + s^2 \times \frac{1-b^2}{b^2} - \frac{4s \times \sqrt{1-a^2}}{b}}$$

$+ \frac{SB^3 \times \text{tang. } ASH}{6V} \times \overline{\text{cos. } ASH + \text{sec. } ASH}$; in which expression $SA = \frac{t \times \sqrt{b}}{\sqrt{b} + \sqrt{ac}}$, and $SB = \frac{t \times \sqrt{ac}}{\sqrt{b} + \sqrt{ac}}$; t being = the breadth BA.

The value of ET having been thus obtained, if $ER = d \times$ sine ASH be subtracted from it, there will remain the value of GZ, the measure of the vessel's stability.

Suppose the vessel's inclination from the perpendicular, or ASH, to be = 15°, let the inclination of the sides inward above the water-line, from the direction of the parallel sides under the water, or HAQ = 15°; therefore SAH = 75°, and SHA = 90°, making BA = t, and, applying these conditions to the analytical value just determined, it is found that KL = 65.530; the area ASH = 323.42; and the volume immersed, or V, being assumed = 3600, as in the preceding cases, $ET = \frac{65.524 \times 323.42}{3600}$ = 5.89. Subtracting from this, ER = 3.36, there will remain GZ = 2.53, or the measure of stability. If the vessel's weight should be 1000 tons, the force of stability will be 1000 tons, acting to turn the vessel at a distance of $\frac{2.53}{100}$ parts of the breadth BA from the axis; which is equal to a force or weight of $\frac{1000 \times 2.53}{50}$ = 50.6 tons, acting to turn the vessel at a distance of 50 from the axis.

CASE IV.

The sides of a vessel project outwards, and at equal inclinations to the plane of the masts, both above and beneath the water-line.

BA (Tab. IX. fig. 7.) is the breadth of the vessel, and coin-

cides with the water's surface, when the vessel floats upright. XE denotes the plane of the masts, bisecting BA in the point S. PU, QW, are lines drawn through the extremities of the line BA, and perpendicular to it, and therefore parallel to EX: the sides of the vessel above the water-line, AH, BY, are inclined outward from the plane of the masts, at an angle QAH = PBY; and BC, AD, are the sides under the water-line, also inclined outward from the plane of the masts, at an angle = DAW = CBU = QAH. G and E represent the centres of gravity of the vessel, and of the volume displaced, as in the former cases. To construct the measure of stability, corresponding to any given angle of inclination from the upright,

Through the point S, which bisects the line BA, draw the line CH inclined to BA, at the angle ASH, equal to the given angle of inclination from the upright. Since, by the conditions of this problem, the triangles ASH, BSC, are similar and equal figures, it follows, that when the vessel is inclined from the perpendicular through the angle ASH, it will be intersected by the water's surface, in the direction of the line CH. The subsequent part of this construction is similar to those of the preceding cases, as sufficiently appears by inspection of the figure.

Let the breadth of the vessel at the water's surface, or BA $= t$: put the sine of the angle ASH $= s$, sine SAH $= a$, sine SHA $= b =$ radius $= 1$, GE $= d$. Then the area ASH, or BSC $= \frac{t^2 sa}{8b}$, and, if the total volume immersed is put $=$ V, the measure of the vessel's stability, or GZ, will be $= \frac{t^3 sa}{24Vb} \times \sqrt{4 + s^2 \times \frac{1-b^2}{b^2} + \frac{4s \times \sqrt{1-a^2}}{b}} - d\,s.$

Let BA, or $t = 100$, $d = 13$, ASH $= 15°$, SAH $= 105°$, V $= 3600$, as in the former cases: then, $s =$ sine $15°$, $a =$ sine $105°$, $b =$ sine $60°$; by referring to the solution, GZ $= 3.59$; and the stability will be the weight of the vessel, suppose 1000 tons, acting at the distance 3.59 from the axis, to turn the vessel; which force is equivalent to a weight of 71.7 tons, applied at the distance of 50 from the axis.

CASE V.

The sides of a vessel are inclined inward, and at equal angles of inclination to the plane of the masts, both above and beneath the water-line.

B A (fig. 8.) is the breadth of the vessel coinciding with the water's surface, when floating upright. XE represents the plane of the masts, bisecting B A in the point S. U P, WQ, are lines drawn through the extremities of the line B A, parallel to X E. BY, A H, are the sides of the vessel above the water-line, inclined inward to the plane of the masts, at the angle QAH = YBP. B C, AD, are the sides under the water-line, inclined inward to the plane of the masts, at the angle DAW or CBU, which are equal to HAQ or YBP. The other conditions are as in the former cases. Through the point S, draw the line C H inclined to B A, at the angle A S H, equal to the vessel's inclination from the upright. Since the triangles ASH, BSC, are similar and equal figures, it follows, that when the vessel is inclined to the angle A S H, it will be intersected by the water's surface in the line C H. The remaining part of this construction is similar to that of the preceding cases.

The same notation being adopted with that which was used

in the preceding case, by referring to trigonometrical properties, it is found that the measure of stability, or

$$GZ = \frac{t^3 sa}{24\sqrt{b}} \times \sqrt{4 + s^2 \times \frac{1-b^2}{b^2} - \frac{4s \times \sqrt{1-a^2}}{b}} - ds.$$

Let $t = 100$, $d = 13$, the inclination of the sides inward, or HAQ $= 15°$, ASH $= 15°$, SAH $= 75°$, SHA $= 90°$: by calculating from these data, it is found that GZ $= 2.21$.

If the vessel's weight should be 1000 tons, the stability will be this weight, acting to turn the vessel at the distance 2.21 from the axis; which is equivalent to a force of 44.2 tons, applied at the distance of 50 from the axis.

CASE VI.

The sides of a vessel coincide with the sides of an isosceles wedge, (fig. 9.) meeting, if produced, in an angle BWA, which is beneath the water's surface.

Supposing the sides to be continued till they meet, the vertical sections will be equal isosceles triangles. BAW represents one of these triangles, BA being coincident with the water's surface, and cutting off the line BW equal to AW. The angle WBA $=$ WAB is supposed to be given. If the vessel should be inclined from the perpendicular, so that the water's surface shall coincide with the line CH, the point of intersection S must be so situated, that the area or volume immersed, in consequence of the inclination, that is, ASH, shall be equal to the area or volume SBC, which has emerged from the water. Previously, therefore, to the construction of this case, the position of the point S is to be geometrically determined, according to the conditions required.

Let BWA (fig. 10.) represent a vertical section of the vessel. Through the extremity B of the line BA, draw BO inclined to BA, at the angle ABO, equal to the vessel's inclination from the upright. In this line, take any point R, and in BR take BI to BR, as the sine of the angle WBR to radius. Also take BF to BR as the sine of BRW to radius; and let BG be a geometrical mean proportional between the lines BF and BI; from the point G, set off GZ equal to BF; join ZA, and, through G, draw GS parallel to ZA; and, through S, draw CH parallel to BZ. The area ASH will be equal to the area SBC.

By the construction, the angle ARB = AHS, and the angle WCH = WBR;

also BR : BI : : rad. : sine SCB,
and BF : BR : : sine AHS : rad.

Joining these ratios, BF : BI : : sine AHS : sine SCB.

By the construction, and the similarity of the triangles BGS, BZA.

$$BF : BI :: BF^2 : BG^2 :: GZ^2 : BG^2 :: SA^2 : SB^2.$$

Wherefore $SA^2 : SB^2$: : sine AHS : sine SCB

By trigonometry, SH : SA : : sine SAH : sine AHS

and SB : SC : : sine SCB : sine SAH = sine SBC

Joining these ratios, SA × SH : SB × SC : : 1 : 1.

Therefore SA × SH = SB × SC.

But the angle ASH being equal to the angle BSC, the area of the triangle ASH will be to the area of the triangle BSC, as SA × SH is to SB × SC; and, since SA × SH is equal to SB × SC, the area of the triangle ASH is equal to the area of the triangle SBC.

The point S having been thus determined, (fig. 9.) if the line CH is drawn through it, inclined to BA at an angle equal to the vessel's inclination from the upright, the water's surface will coincide with the line CH.

To proceed with the construction of this case; bisect BA in D, (fig. 9.) and join WD: let G represent the centre of gravity of the vessel, and E the centre of gravity of the volume displaced, when the vessel floats upright. Let M and I be the centres of gravity of the triangles SAH, SBC; and ML, IK, lines drawn perpendicular to CH, through the points M and I respectively. Through G, draw GU parallel to CH; and, through E, draw EV parallel and equal to KL. In EV, take ET to EV as the area ASH is to the area representing the total volume immersed. Through T, draw TZ perpendicular to GU. GZ will be the measure of the vessel's stability.

As in the preceding cases, let BA be denoted by the letter t, and put the sine of ASH $= s$, sine SAH $= a$, sine SHA $= b$, sine SCB $= c$; the total volume immersed $=$ V.

By trigonometry, $SL = \frac{SA}{3} \times \sqrt{4 + \frac{s^2 \times \overline{1 - b^2}}{b^2} + \frac{4s \times \sqrt{1 - a^2}}{b}}$

$$SK = \frac{SB}{3} \times \sqrt{4 + \frac{s^2 \times \overline{1 - c^2}}{c^2} - \frac{4s \times \sqrt{1 - a^2}}{c}}$$

And, since the area ASH $= \frac{SA^2 \times sa}{2b} = \frac{SB^2 \times sa}{2c}$, and V is the area representing the entire volume immersed, the measure of stability, or

$$GZ = \frac{SA^3 \times sa}{6Vb} \times \sqrt{4 + \frac{s^2 \times \overline{1 - b^2}}{b^2} + \frac{4s \times \sqrt{1 - a^2}}{b}}$$
$$+ \frac{SB^3 \times sa}{6Vc} \times \sqrt{4 + \frac{s^2 \times \overline{1 - c^2}}{c^2} - \frac{4s \times \sqrt{1 - a^2}}{c}} - ds.$$

In which expression, $SA = \frac{t \times \sqrt{b}}{\sqrt{b} + \sqrt{c}}$, and $SB = \frac{t \times \sqrt{c}}{\sqrt{b} + \sqrt{c}}$.

Let the sides of a vessel be plane surfaces, inclined to each other at an angle of 30°; the vessel's inclination from the upright $=15°$; BA $=t=100$; GE $=d=13$; the angle SAH $=105°$; AHS $=60°$: BCS $=90°$. By calculating the value of the line GZ, according to the solution just given, it is found

$$\text{that } \frac{SA^3 \times sa}{6Vb} \times \sqrt{4 + \frac{s^2 \times \overline{1-b^2}}{b^2} + \frac{4s \times \sqrt{1-a^2}}{b}} = 3.1155$$

$$\text{and } \frac{SB^3 \times sa}{6Vc} \times \sqrt{4 + \frac{s^2 \times \overline{1-c^2}}{c^2} - \frac{4s \times \sqrt{1-a^2}}{c}} = 3.1075$$

Sum of these values $= 6.2230$

ds - - - $= 3.365$

Finally, the measure of the vessel's stability,

$$\text{or } \frac{SA^3 \times sa}{6Vb} \times \sqrt{4 + \frac{s^2 \times \overline{1-b^2}}{b^2} + \frac{4s \times \sqrt{1-a^2}}{b}}$$

$$+ \frac{SB^3 \times sa}{6Vc} \times \sqrt{4 + \frac{s^2 \times \overline{1-c^2}}{c^2} - \frac{4s \times \sqrt{1-a^2}}{c}} - ds = GZ = 2.858$$

If the weight of the vessel should be 1000 tons, the force of stability will be equivalent to that weight of pressure, acting at the distance of 2.85 from the axis; or the weight of 57.0 tons, acting at the distance of 50 from the axis.

If the sides should be inclined at an angle of 60°, instead of 30°, the measure of stability will be 2.92; and the effort to turn the vessel equal to 1000 tons, acting at the distance 2.92, or 58.4 tons acting at the distance of 50 from the axis.

The sides of vessels are not unfrequently formed so as to coincide with the sides of an isosceles wedge, or are so little curved as to approximate nearly to that figure, at least so far as that portion of the sides extends which may be immersed in, or may emerge from, the water, by the vessel's inclination. The preceding solution being expressed in terms which are rather

complicated, another solution is subjoined, by which the measure of stability is exhibited in more simple terms. The investigation is troublesome; but the conciseness of the result, and the readiness with which it is applied to practical cases, compensate for the difficulty of obtaining it.

Let the isosceles triangle BAF (Tab. X. fig. 11.), represent a vertical section of the vessel; the base of which, BA, coincides with the water's surface, when the vessel floats upright. Bisect BA in D, and join FD. Let G be the centre of gravity of the vessel, and take FE to FD as 2 to 3; E will be the centre of gravity of the immersed volume when the vessel floats upright. Draw the line CH, * intersecting the line BA at an angle ASH, equal to the given angle of the vessel's inclination from the perpendicular, and cutting off the area ASH equal to the area BSC. When the vessel is inclined through the angle ASH, the line of intersection with the water's surface will coincide with CH. Bisect CH in the point N, and join FN: take FQ to FN as 2 to 3. Q is the centre of gravity of the area CFH, representing the volume immersed when the vessel is inclined. Through Q, draw QM perpendicular to CH; and, through G, draw GZ perpendicular to QM. GZ is evidently the measure of the vessel's stability.

To obtain an analytical value of the line GZ, through Q, draw OQP parallel to CH; through G, draw GR parallel to QM; and, through E, draw ET perpendicular to QM. In this investigation it will be expedient, first, to express in general and known terms the line FW; secondly, the line WQ, which is to MW as the sine of the vessel's inclination to radius: this will give the value of MW, which being added to WF before

* By the construction, p. 228.

found, the sum will be the line FM; from which, if FE, or $\frac{2}{3}$ of FD, be subtracted, there will remain the line ME; which is to ET as radius is to the sine of the inclination EMT, or ASH. ET will therefore be expressed in known terms; from which, if ER be subtracted, the remaining line will be RT, or GZ, the measure of the vessel's stability, analytically expressed.

By the construction, the area FBA is equal to the area FCH; and, since the area BAF is to the area IKF in the same * proportion which the area FCH bears to the area FOP, it follows, that the area FIK is equal to the area FOP. Also, because CN is equal to NH, and OP is parallel to CH, it follows, that OQ is equal to QP. For brevity, let the angle KYP, or ASH, be denoted by the letter S; FPO = FHC by P; POF = HCF by O; also let the angle PFO be made = F.

Because the areas IFK, PFO, are equal,

$\frac{FO \times FP \times \text{sine } F}{2} = \overline{FE}^2 \times \text{tang.} \frac{1}{2} F$, radius being $= 1$: wherefore,

$FP = \frac{2\, FE^2 \times \text{tang.} \frac{1}{2} F}{FO \times \text{sine } F} = \frac{FE^2 \times \text{sec.}^2 \frac{1}{2} F}{FO}$; and, because

$FO = \frac{FP \times \text{sine } P}{\text{sine } O}$, by substitution $FP^2 = \frac{FE^2 \times \text{sec}^2 \frac{1}{2} F \times \text{sine } O}{\text{sine } P}$;

and therefore $FP = FE \times \text{sec.} \frac{1}{2} F \times \sqrt{\frac{\text{sine } O}{\text{sine } P}}$: but sine FWP = cos. S.

Wherefore, FW : FP : : sine P : cos. S; or

$FW : FE \times \text{sec.} \frac{1}{2} F \times \sqrt{\frac{\text{sine } O}{\text{sine } P}}$: sine P : cos. S; consequently,

$FW = \frac{FE \times \text{sec.} \frac{1}{2} F \times \sqrt{\text{sine } O \times \text{sine } P}}{\text{cos. } S}$.

By investigation,† founded on the rules of trigonometry, it

* Each of these proportions being as 9 to 4.

† See Appendix.

appears that $\sqrt{\text{sine O} \times \text{sine P}} = \frac{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}}{\text{sec.} \frac{1}{2} F \times \text{sec. S}}$:
which quantity being substituted instead of $\sqrt{\text{sine O} \times \text{sine P}}$, in the value of FW just found, the result will be

$$FW = FE \times \sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S}.$$

It is found also, from trigonometrical rules, that

$$* \; WQ = FE \times \frac{\text{tang.}^2 \frac{1}{2} F \times \text{tang. S} \times \text{sec. S}}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}},$$ and since

WQ : WM : : sine S : rad. we have

$$WM = \frac{FE \times \text{tang.}^2 \frac{1}{2} F \times \text{tang. S} \times \text{sec. S}}{\text{sine S} \times \sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}},$$ or because

$\frac{\text{tang. S}}{\text{sine S}} = \text{sec. S}$, $WM = FE \times \frac{\text{tang.}^2 \frac{1}{2} F \times \text{sec.}^2 S}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}}$; and, since $FW = FE \times \sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}$, and $FM = WF + WM$, we obtain the value of $FM = FE \times \frac{\text{sec.}^2 \frac{1}{2} F}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang}^2 S}}$:

Therefore $ME = FM - FE = FE \times \overline{\frac{\text{sec.}^2 \frac{1}{2} F}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}} - 1}$,

and $ET = FE \times \text{sine S} \times \overline{\frac{\text{sec.}^2 \frac{1}{2} F}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}} - 1}$.

This value of ET is inferred from supposing the area BFA to represent the entire volume immersed, and which $= \frac{t^2}{4 \times \text{tang.} \frac{1}{2} F}$ t being equal to the line BA.

If, the sides BC, AH, remaining the same, the figure and magnitude of the immersed volume should be changed, so as to be represented by any other quantity V†, the line ET will be increased or diminished in the inverse proportion of the entire volumes immersed, that is

as $V : \frac{t^2}{4 \times \text{tang.} \frac{1}{2} F} : : FE \times \text{sine S} \times \overline{\frac{\text{sec.}^2 \frac{1}{2} F}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}} - 1} : ET.$

And, since $FE = \frac{t}{3 \text{ tang.} \frac{1}{2} F}$,

* See Appendix.

† See pages 213 and 214.

$$ET = \frac{t^3 \times \text{sine } S}{12V \times \text{tang.}^2 \frac{1}{2} F} \times \overline{\frac{\text{sec.}^2 \frac{1}{2} F}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}} - 1};$$

and the measure of the vessel's stability, expressed in general and known terms, will be

$$*GZ = \frac{t^3 \times \text{sine } S}{12V \times \text{tang.}^2 \frac{1}{2} F} \times \overline{\frac{\text{sec.}^2 \frac{1}{2} F}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}} - 1} - d \times \text{sin. } S.$$

When the angle of inclination S is evanescent, or in a practical sense very small, the expression becomes

$GZ = \frac{t^3 \times \text{sine } S}{12V} - d \times \text{sine } S$, agreeing with the solution given by M. Euler † in this particular case.

If the inclination of the sides BF, AF, should be evanescent, the sides will become parallel to each other, and to the masts, both above and beneath the water-line; a case which has already been solved ‡: and consequently, the solution of case 1. ought to agree with that which has been just given for the stability, when the two sides are inclined at a given angle, assuming that angle as evanescent. Assuming, therefore, the angle BFA evanescent, and S of any finite magnitude in the general value of GZ, above determined, we have

$\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S} = \frac{2 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}{2}$, and

$$\frac{\text{sec.}^2 \frac{1}{2} F}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}} = \frac{2 + 2 \times \text{tang.}^2 \frac{1}{2} F + \text{tang.}^2 \frac{1}{2} F \times \text{sec.}^2 \frac{1}{2} F \times \text{tang.}^2 S}{2};$$

and therefore $\S GZ = \frac{t^3 \times \text{sine } S}{12V \times \text{tang.}^2 \frac{1}{2} F} \times \frac{\text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S + 2 \times \text{tang.}^2 \frac{1}{2} F}{2} - d \times \text{sin. } S$.

* This expression for the measure of stability, is evidently more simple, and better adapted to practical application, than that which is inserted in page 229. The present result might perhaps be obtained by more concise methods: the investigation here given is the best that occurred to the author, after repeatedly endeavouring to discover some other, requiring fewer trigonometrical calculations.

† Theory of the Construction and Properties of Vessels, chap. viii. ‡ Case 1.

$$\S\ ET = \frac{t^3 \times \text{sin. } S}{12V \times \text{tang.}^2 \frac{1}{2} F} \times \overline{\frac{2 + 2 \times \text{tang.}^2 \frac{1}{2} F + \text{tang.}^2 \frac{1}{2} F \times \text{sec.}^2 \frac{1}{2} F \times \text{tang.}^2 S}{2} - 1},$$

or because $\text{sec. } \frac{1}{2} F = 1$, $ET = \frac{t^3 \times \text{sin. } S}{12V} \times \frac{2 + \text{tang.}^2 S}{2}$.

$$\text{or } GZ = \frac{t^3 \times \text{sine } S}{24V} \times \overline{\text{tang.}^2 S + 2} - d \times \text{sin. } S,$$

$$\text{or } GZ = \frac{t^3 \times \text{tang. } S}{24V} \times \overline{\text{cos. } S + \text{sec. } S} - d \times \text{sin. } S,$$

which is the measure of stability, when the inclined sides AF, BF, become parallel, the angle F vanishing. But this quantity is the measure of stability when the sides are parallel, as determined by direct investigation*; by which agreement the consistency of the two solutions is evinced.

To exemplify the general solution for the case of the sides inclined at a given angle, suppose the angle BFA to be $30° = F$, let $S = 15°$, $AB = t = 100$, $GE = d = 13$, $V = 3600$.

From the analytical value of the line GZ, we obtain

$\frac{t^3 \times \text{sine } S}{12V \times \text{tang.}^2 \frac{1}{2} F}$ - - -	$= 83.4464$
$\frac{\text{sec.}^2 \frac{1}{2} F}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}} - 1$ - -	$= .07457$
$83.4464 \times .07457$ -	$= 6.223$
$d \times \text{sin. } S$ - -	$= 3.365$
and GZ, the measure of stability	$= 2.858,$

precisely agreeing with the result calculated by the solution, in pages 229 and 230, which has no apparent similitude or relation to the value for stability, as expressed according to this last investigation, which is

$$GZ = \frac{t^3 \times \text{sin. } S}{12V \times \text{tang.}^2 \frac{1}{2} F} \times \overline{\frac{\text{sec.}^2 \frac{1}{2} F}{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}} - 1} - d \times \text{sin. } S.$$

According to the solution in page 229, the measure of stability is

$$GZ = \frac{SA^3 \times sa}{6Vb} \times \sqrt{4 + \frac{s^2 \times \overline{1 - b^2}}{b^2} + \frac{4s \times \sqrt{1 - a^2}}{b}}$$

$$+ \frac{SB^3 \times sa}{6Vc} \times \sqrt{4 + \frac{s^2 \times \overline{1 - c^2}}{c^2} - \frac{4s \times \sqrt{1 - a^2}}{c}} - ds,$$

* Case 1.

in which value $s = \text{sin. S}$; $a = \text{sin. SAH}$; $c = \text{sin. SCB}$; $\text{SA} = \frac{t \times \sqrt{b}}{\sqrt{b} + \sqrt{c}}$, and $\text{SB} = \frac{t \times \sqrt{c}}{\sqrt{b} + \sqrt{c}}$. It might not, perhaps, be easy to deduce either of these values from the other, or to demonstrate their equality, otherwise than by the separate investigations from which they have been inferred; and yet these quantities are not approximations to equality, but are strictly and mathematically equal.

CASE VII.

The sides of a vessel are coincident with the sides of a wedge, meeting, if produced, at an angle which is above the water's surface.

The sides of a vessel are represented by the lines *qb*, *cd*, (fig. 12.) inclined at an angle, so as, if produced, to meet at the point *w* above the water's surface, which is coincident with *ba*; the lines *wa*, *wb*, are assumed equal. Suppose the vessel to be inclined from the perpendicular through any given angle; let a line *cb* be drawn, intersecting the line *ba* at the given angle of inclination, and cutting* off the area *asb* equal to the area *bsc*: when the vessel is inclined to the given angle from the upright, the water's surface will be coincident with the line *cb*. Let *m* and *i* represent centres of gravity of the areas *asb*, *bsc*, respectively, and let the line *kl* be constructed as in the former cases. Let *g* be the centre of gravity of the vessel, situated in the line *we*, which is drawn perpendicular to and bisects *ba*, and let *e* be the centre of gravity of the volume displaced; making *ev* parallel and equal to *lk*, take *et* to *ev* as the area *bsc* is to the area representing the entire volume immersed. Through *g*, draw *gu* parallel to *cb*, and, through *t*, draw *tz* perpendicular to *gu*. *gz* will be the measure of the vessel's stability.

* Page 228.

From this construction, the following proposition is to be inferred.

The sides of a vessel are plane surfaces, represented, (fig. 9.) when produced, by the equal lines AW, BW, which meet in the point W, beneath the water-line. The sides of another vessel (fig. 12.) are also plane surfaces inclined to each other at the same angle as in the former case, and represented by the equal lines $a\,w$, $b\,w$, which meet at the point w above the water-line: suppose the breadth of both vessels to be equal at the water-line, and the angle BWA = the angle $b\,w\,a$; if the distances between the centres of gravity of the vessels and of the immersed volumes are equal, and the weights of the vessels are also equal, the proposition affirms, that the stabilities of the two vessels, when inclined to the same angle from the upright, will always be equal.

Since the line BA = $b\,a$, and the angle BAW = the angle $b\,a\,w$, (fig. 9 and 12.) by the conditions of the proposition, if the angle BAW be applied over the angle $b\,a\,w$, the point A coinciding with the point a, it follows, that the point W, and the point B, must coincide with the point w and the point b respectively; and, since the lines BA, $b\,a$, are divided in the points S, s, on the same conditions, namely, so that the lines CH, $c\,h$, shall be inclined to BA, and $b\,a$, at the same angle, and shall cut off the areas ASH, $a\,s\,h$, equal respectively to the areas BSC, $b\,s\,c$; it must follow, that when the line AB is applied so as to coincide with the line $a\,b$, the point S will coincide with the point s; and the angle ASH being equal to the angle $a\,s\,h$, by the supposition, the line SH will be equal to the line $s\,h$; and the triangle ASH will be equal and similar to the triangle $a\,s\,h$. The centres of gravity of these triangles, therefore, or the points M and m, will coincide, as will also the lines ML, $m\,l$, which

are drawn through these points perpendicular to CH and cb. The line SL will therefore coincide with the line sl, and is equal to it. In the same manner, it is proved that the line SK is equal to the line sk; consequently, KL is equal to kl. And since, by construction, the area ASH is equal to the area BSC, and the area asb equal to the area bsc; and, on application of the figure AWB to the figure awb, the triangle ASH coincides with the triangle asb, it follows, that the four areas ASH, asb, BSC, bsc, are all equal.

But $ET^* = \frac{KL \times \text{area ASH}}{\text{total volume immersed}}$, and $et\dagger = \frac{kl \times \text{volume } asb}{\text{total volume immersed}}$; and, since $KL = kl$, and the volume ASH = the volume asb, $KL \times$ volume ASH $= kl \times$ volume asb; and the entire volume immersed being the same in both vessels, by the supposition, it follows that $ET = et$.

This equality between the lines ET, et, is independent of the position of the centres of gravity of the vessels, G, g, and also of the position of the centres of gravity, E, e, in the lines WD, wd. If the distances of GE, ge, should be equal, since the angles of inclination from the upright, or EGR, egr, are equal by the supposition, it follows that the sines of those angles to equal radii must be equal, or $ER = er$. Subtracting, therefore, ER from ET, and er from et, the remaining lines RT, rt, must be equal, or $GZ = gz$. The stability, therefore, of a vessel, the sides of which are inclined to an angle under the water's surface, is equal to the stability of the vessel of which the sides are inclined to an angle which is above the water's surface: the breadth at the water-line, and the other conditions, being the same in both vessels.

This proposition is not confined to the case here demon-

* Fig. 9. See page 212. † Fig. 12. See page 212.

strated, being equally true, whatever figure be given to the sides; and whether they are plane or curved, provided the sides under the water-line in one vessel are similar and equal, and similarly disposed, in respect of the water-line, to the sides of the other vessel above the water-line. QC, HO, (fig. 13.) represent the sides of a vessel projecting outward above the water-line, and inclined inward under the water-line. Suppose the vessel to be inclined from the upright through any given angle, and let CH be supposed drawn inclined to the line BA at the given angle, and cutting off the area ASH equal to the area SBC: when the vessel is inclined, the water's surface will coincide with the line CH.

Let the sides QC, OH, be conceived to revolve round the line BA as an axis, through 180°; the position of the sides will be reversed, as represented in fig. 14: the sides which projected outward above the water-line (fig. 13.) equally project outward under the water-line in fig. 14. and are similarly situated in respect to the water-lines BA, *ba*. In like manner, the sides which are inclined inward under the water-line, in fig. 13. are equally inclined inward above the water-line in fig. 14.; and are also similarly situated in respect to that line. If M, I, are the centres of gravity of the areas ASH, BSC, and *m*, *i*, the centres of gravity of the areas *asb*, *bsc*, as in the former cases, and perpendicular lines be drawn through them, ML, IK, and *ml*, *ik*; by arguments similar to those which were used to demonstrate the preceding proposition, it will be evident that the lines KL, *kl*, are equal; also that the areas ASH, BSC, *asb*, *bsc*, are all equal: and, by proceeding to construct the measures of stability corresponding to the two cases, it will appear that $GZ = gz$; the weight of both vessels, and consequently the entire volumes immersed under water, being the same. The conclusion is, that, the

other conditions remaining the same, if the position of the sides should be reversed, in the manner described in the proposition, the stability, at equal angles of inclination, will remain the same.

It may be proper in this place to remark, that the metacentric curve, described by M. Bouguer,* and M. Clairbois,† and applied to the preceding cases, does not appear to have any relation to the stability of vessels, except in the single point where the curve intersects the vertical axis; and therefore can be applicable only in the case when the angle of the vessel's inclination from the upright is evanescent. Let FBC, DAH, (fig. 15.) represent the sides of a vessel, BA coinciding with the water's surface when the vessel floats upright: bisect BA in S, and draw ISE perpendicular to BA. Let E be the centre of gravity of the volume immersed. Suppose the vessel to be inclined through a very small angle AS *a*, so that the water's surface shall now coincide with the line *ba*; and let the centre of gravity of the volume immersed be Q. Through Q, draw the line QW*z* perpendicular to *ba*, intersecting the line IE in the point W. This point is called by M. Bouguer the metacentre. One of the principal properties of this point is, that whenever the centre of gravity of the vessel is situated beneath it, any where in the line WE, (suppose at G,) the vessel will float permanently, with the line IE vertical; but that, if the centre of gravity is placed above the metacentre, suppose at *g*, the vessel will overset, from that position; for, drawing GZ, *gz*, perpendicular to Q*z*, if the vessel should be inclined through a small angle AS *a*, so as to immerse the portion of the side A*a*, the force of pressure acting in the direction of the line QZ, to turn the vessel round an axis passing horizontally through G, will elevate the parts adjacent to A, so as to restore the upright position: whereas, if the centre of gravity

* *Traité du Navire, p.* 270.

† Clairbois, p. 289, *et seq.*

should be placed above the metacentre, suppose at *g*, the same force of the fluid's pressure, by turning the vessel round an axis passing through *g*, must immerse further the portion of the side A*a*; and this immersion, being continued, will cause the vessel to overset. Another property of this point has been demonstrated by M. EULER,* and other authors; which is, that when the angles of a vessel's inclination are evanescent, or very small, the effect of stability, to restore the vessel to the upright position, will be as the sine of the angle of inclination GWZ † and the line WG jointly: at the same small angles of inclination, the stability of different vessels will be in proportion to the line WG, or distances of the metacentre above the centre of gravity.

Let the curve EQ*q* (Tab. XI. fig. 16.) represent the line traced by the successive centres of gravity of the immersed volumes, while the vessel is inclined from the upright through any angle ASH. M. BOUGUER demonstrates, that a tangent to this curve in any point Q, will be parallel to the water's surface CH, corresponding to that point: if, therefore, through any two adjacent points Q and *q*, in the curve EQ*q*, lines QM, *q*N, are drawn perpendicular to the lines CH, *cb*, respectively, the intersection of those lines in the point X will be the centre of curvature, and XQ, X*q*, will be the radii of a circle, which has the same curvature with the curve EQ in the point Q. For the same reasons, the line WE (fig. 15, 16.) is the radius of a circle which has the same curvature with the curve EQ in the point E. The point W has been denominated the metacentre corresponding to the upright position of the vessel, when the line WGE is perpendicular to the water's surface. M. BOUGUER denomi-

* Theory of the Construction of Vessels, chap. 8. book i. † To radius = 1.

nates the point X the metacentre corresponding to the position when the vessel has been inclined from the upright through the angle ASH; and the curve WX is termed the metacentric curve, being the line traced by the successive metacentres, or intersections, of the lines QM, q N, drawn perpendicular to the lines in which the vessel is intersected by the water's surface, while it is gradually inclined. Consequently, according to this construction, the metacentric curve WX is the evolute, of which the curve EQ q is the involute.

The construction and properties of the metacentric curve being a subject of geometrical reasoning, considered purely as such, are liable neither to ambiguity nor error; but, on what grounds these properties are applied to measure the stability of vessels, or to estimate their security from oversetting, when much inclined from the upright, is not explained by M. BOUGUER, M. CLAIRBOIS, or any other author I have had an opportunity of consulting: yet the opinions expressed by these authors on the subject in question, have been adopted by many persons as established principles; and, being of some importance in the practice, as well as theory, of naval architecture, it cannot be thought superfluous to pay some farther attention to them.

M. BOUGUER,* having demonstrated the property of the metacentre, which gives security from spontaneously oversetting, to a vessel, whenever the centre of gravity is situated beneath it, proceeds to observe, that his theorem, being founded on supposing the angles of the vessel's inclination as evanescent, or extremely small, such as a vessel may experience in smooth water, cannot be relied on for ascertaining the safety of ships,

* *Traité du Navire*, p. 269.

when agitated by the winds and waves in open sea, where the inclinations from the upright must often become considerable. In order to extend the application of his theorem to the larger angles of inclination, he proposes to examine whether the metacentre ascends or descends as the vessel is gradually inclined.* To effect this, the curve line EQ*q* (fig. 16.) is to be traced, by finding the successive centres of gravity of the volumes immersed while the vessel is inclined; and, from this curve the metacentric curve WX is to be defined: the point where the metacentric curve meets the vertical axis in W, is the metacentre corresponding to the position when the vessel floats upright and quiescent. He observes, that if the metacentre X ascends from its original position W, while the vessel is inclined gradually from the perpendicular, the vessel will be secure from oversetting; but will be insecure, if that point should descend while the vessel is inclined. No demonstration of this proposition is given, either by M. Bouguer, or by M. Clairbois, who undertakes to explain the principles delivered in this chapter of M. Bouguer's work.† If the proposition has been suggested by some analogies which subsist between the construction of the lines EW, QX, and other lines similarly drawn, they will be insufficient to establish the truth of it. The analogies are such as the following. W being the metacentre, and E the centre of gravity of the volume displaced, when the vessel floats upright, WE is the radius of curvature to the curve EQ *q*, at the point E; X being also the metacentre, constructed according to the method which has been described, when the vessel has been inclined through an angle ASH, and Q the centre of gravity of the corresponding volume immersed; XQ is the

* *Traité du Navire*, p. 271. † Clairbois *sur l'Architecture Navale*, p. 289, *et seq.*

radius of curvature of the curve EQ at the point Q. Also, EW is perpendicular to the water's surface AB, when the vessel floats upright; and XQ is perpendicular to the water's surface, when the vessel is inclined through the angle ASH. When the vessel floats upright, the stability is measured by the sine of inclination and the line GW jointly; and therefore the angle of inclination being given, will be measured by the line GW, and will depend in some ratio or proportion on the line EW, when GE remains the same, or when G is made to coincide with E.

The question is, whether the stability, when the vessel is inclined to the angle ASH, will depend in a similar degree on the line QX? Respecting the supposed analogy it may be remarked, that one condition absolutely necessary to establish it is wanting; namely, the centre of gravity G ought to be situated in the line XQ; but it is considerably distant from that line, being placed in the vertical axis of the vessel WGE. This material difference in the conditions corresponding to the two cases, is sufficient to destroy all inference from analogy, even if arguments of this kind could be admitted, in geometrical subjects, to supply the place of demonstration. It is not difficult to shew geometrically, in what position and circumstances of the vessel the line XQ will be the correct measure of its stability. Suppose that, by any alteration in the distribution of the ballast or lading, the centre of gravity should be removed from the line WGE to the line XGQ, the vessel will float permanently with the line XQ perpendicular to the horizon, and the mast WE will be inclined to it at the angle = ASH. Since XQ is the radius of curvature of the curve EQ at the point Q, and is also perpendicular to CH, the point X will be the true position of the metacentre, corresponding to the float-

ing position of the vessel, when the centre of gravity is situated out of the vertical axis in the line XQ, and Q is the centre of gravity of the volume displaced. The measure of stability, when the inclination is any small angle, will be the sine of that angle and the line XG jointly; comparing, therefore, the stability of the vessel when the centre of gravity is situated in the line WGE, with the stability when the centre of gravity is in the line XGQ, the proportion of the two stabilities, at equal small angles of inclination, will be as the line WG is to the line XG; if the centre of gravity G should coincide with the point E in the first case, and with the point Q in the latter case, a condition often adopted by M. BOUGUER, the stabilities will be in the proportion of the lines WE to XQ, or in a triplicate ratio of the lines BA, CH.

Such is the result of the examination proposed, from which the only inference is, that while the centre of gravity remains situated in the vertical axis WE, (the position it occupies in vessels of every description,) the line XQ cannot be assumed to measure or estimate the stability and security of a vessel at sea, when inclined to the larger angles from the upright. M. CLAIRBOIS, to illustrate the principles of M. BOUGUER, adopts two instances, which are the same with Case VI. (fig. 9.) and Case VII. (fig. 12.) in these pages. In the former case, the sides coincide with those of an isosceles wedge: the breadth BA at the water-line being the base, and the angle BWA situated under the water's surface. As the vessel thus formed is gradually inclined from the perpendicular, he shews,* that the curve traced by the centre of gravity of the successive volumes immersed is an hyperbola. Of this curve he calculates the successive radii

* CLAIRBOIS, p. 291. 295.

of curvature, which he demonstrates to increase continually with the inclination of the vessel: he shews, that the centres of curvature thus found, or successive metacentres, according to M. BOUGUER's construction, ascend as the vessel is inclined; a circumstance which, according to his principle, imparts security from oversetting. On the contrary, in the other instance, when the sides of a vessel are inclined to an angle which is above the water's surface, (fig. 12.) from a similar mode of reasoning he concludes, that the metacentre descends as the vessel is more and more inclined; which, according to his proposition, would endanger the safety of the vessel, when inclined to considerable angles.

This determination is evidently inconsistent with the solutions of Case VI. and VII. preceding, by which it appears, that the stability acting to restore vessels thus constructed to the upright position, under the conditions that have been stated, will be precisely the same at all equal inclinations from the upright, whether the sides are inclined at an angle beneath or above the water-line; all the other conditions being the same in both cases.

The solution of these questions being connected with a principle of some consequence in the practice of naval architecture, the preceding observations have been offered with a view of stating distinctly the opinions which are contradictory to the solutions of Case VI. and VII. referring to the authors who have treated on the subject, in order that a judgment may be formed by persons conversant in naval architecture, whether the propositions advanced by M. BOUGUER and M. CLAIRBOIS, or the solutions of Case VI. and Case VII. here given, may be relied on, as founded on the genuine principles of geometry and mechanics; for error must exist on one side or the other.

But, until the demonstrations of the Cases VI. and VII. are shewn to be erroneous, and reasons are produced in support of M. BOUGUER's propositions, which he has delivered without any demonstration, it may be allowable to suppose that his opinions are, in these particular instances, ill founded.

The same principles are extended by M. BOUGUER * to express a general value of the distance between the metacentre, and the centre of the immersed part of the ship, when inclined to any angle: this distance he affirms to be $\frac{\text{Flu.}\,\dot{x} \times \overline{y^3 + v^3}}{3p}$; † in which expression y and v are the parts of the total ordinate of the water-section, (when the vessel is inclined,) at the distance x, measured on the longer axis from the initial point; the proportion of y and v being determined by a line drawn parallel to the axis through the centre of gravity of the section; and p is put for the volume immersed.

When the centre of gravity is situated in the line QX, (fig. 16.) and the angle of inclination very small, the point of intersection of the lines CH, cb, will bisect the ordinate CH: in this case the vessel floats permanently with the line QX vertical, and consequently with the line WE, or plane of the masts, inclined to the horizon at the angle ASH. But the line QX, consistently with the preceding observations, cannot be applied to measure the stability or security from oversetting of a ship, when the centre of gravity is placed in the line WE; that is, in the plane of masts which divides the vessel into two parts perfectly similar and equal; the only situation which the centre of gravity can occupy, according to any mode of construction hitherto practised.

* *Traité du Navire*, p. 273.

† No demonstration is given by M. BOUGUER of this proposition.

A few remarks may be added in this place concerning a theorem delivered by M. BOUGUER,* for measuring the stability of vessels when inclined to evanescent angles from the upright. The theorem is this: "When the lengths of vessels "are the same, the stabilities are as the cubes of the breadths." This theorem seems at first view to stand independent of, and not to require, any subsequent explanation: the author immediately applies it to the discussion of some points respecting the stability of vessels. If any person, relying on the author for the truth of this theorem, should only pay attention to the proposition as it is here expressed, he would entertain an opinion on the subject of stability which is altogether erroneous. M. BOUGUER,† in a subsequent page, gives a satisfactory account of the limitations and restrictions under which the theorem in question is to be understood. He observes, that a restriction ought to be applied to the conditions of this proposition, in order to insure the exact correctness of it; which is, that the whole weight of the vessel shall be concentered in the centre of gravity of the displaced volume; a condition which may be deemed amongst the most extreme cases that can be devised, and such as is rarely known to exist.‡ The vessel's centre of gravity not being supposed coincident with the centre of the displaced volume; M. BOUGUER § gives the true measure of stability when the angles of inclination are

* *Traité du Navire*, p. 299. † Ibid. p. 299 and 300.

‡ In vessels of burden, the freights of which consist principally of iron, or other metallic bodies, or blocks of stone, the vessel's centre of gravity may be so depressed as to coincide with, or even to be situated under, the centre of the immersed volume. But such a disposition causes many inconveniences in the ship's sailing; and is never adopted when it is possible to raise the centre of gravity to a higher position.

§ *Traité du Navire*, p. 300.

evanescent; the only objection to which is, that it stands in the author's page as being explanatory, and illustrative of a proposition before delivered: whereas, it is in fact the real proposition for measuring the horizontal stability of vessels; the proposition it is intended to explain being a particular case of it, and requiring a condition which scarcely ever takes place in the practice of constructing and adjusting ships for sea.

CASE VIII.

The sides of a vessel are parallel to the masts above the water-line, (fig. 17.) and project outward beneath it.

In the second Case, (fig. 4.) the sides project outward above the water-line, and are parallel to the masts under it. In Case VIII. the disposition and form of the sides are the reverse of the form according to Case II. If, therefore, the angle of projection of the sides under the water, according to Case VIII. should be equal to the angle at which the sides project above the water, according to Case II. the other conditions being the same, the stabilities* of the two vessels will be equal, at all equal inclinations from the quiescent position. The solution of this case must of consequence be precisely the same with the solution of CASE II. and need not be here repeated.

CASE IX.

The sides of a vessel are parallel to the masts above the water-line, (fig. 18.) and are inclined inward beneath it.

In this case, the position of the sides is the reverse of that which is described in Case III. (fig. 6.) If, therefore, the angles at which the sides are inclined inward, according to Case IX.

* Proposition subjoined to Case VII. page 237.

under the water-line, should be equal to the angle at which the sides are inclined inward above the water-line, according to Case III. all the other conditions being the same, the stabilities of the two vessels will be equal, at all equal inclinations from the upright. The solution of Case IX. is therefore to be derived from that of Case III.

CASE X.

The sides of a vessel coincide with the surface of a cylinder, the vertical sections being equal circles.

Let QBOAH (fig. 19.) represent a vertical section of the vessel. The surface of the water coincides with the line BA, when the vessel floats upright. Suppose the vessel to be inclined from the quiescent position, through an angle ASH, so that the water's surface shall intersect the vessel's, when inclined, in the line CH. Bisect the line BA in D, and the line CH in Y; and, through the points D and Y, draw OD, FY, perpendicular to the lines BA, CH, respectively, and meeting, when produced, in the point M, which is the centre of the circle. The angle ASH is the inclination of the vessel from the perpendicular; and, being the inclination of the lines BA, CH, which are perpendicular to the lines OM, FM, respectively, the inclination of the lines OM, FM, or the angle DMF, will be equal to the angle ASH. Let E be the centre of gravity of the area BOA, representing the volume displaced, when the vessel floats upright, and quiescent. In the line MF, take MQ equal to ME; Q will be the centre of gravity of the area CFH, representing the volume displaced when the vessel is inclined. Let G be the centre of gravity of the vessel; and, through E, draw ET perpendicular to MF; and, through G, draw GZ perpendicular to MF, intersecting that line in the

point Z: GZ is the measure of the vessel's stability. For, since Q is the centre of gravity of the volume immersed, when the vessel is inclined, and the line MF is drawn through it, perpendicular to the water's surface CH, QM will be the direction in which the pressure of the fluid acts, to turn the vessel round an axis passing through G; and GZ, being the perpendicular distance of this line from the centre of gravity, will be the measure of the vessel's stability.

Let the sine of the angle of the vessel's inclination ASH, or OMF, be represented by the letter s to radius $= 1$: by the properties of the circle $ME = \frac{2DA^3}{3 \times \text{area BOA}} = \frac{BA^3}{12 \times \text{area BOA}}$; if, therefore, BA be made $= t$, $ME = \frac{t^3}{12 \times \text{area BOA}}$; and ET $= \frac{t^3 s}{12 \times \text{area BOA}}$.

The area representing the volume displaced is here considered as entirely circular: but if it should be of that form only to the extent of the sides *AH, BC, the remaining part of the area being of any other figure, and the whole area under water should be denoted by V, the line ET will be $= \frac{t^3 s}{12 \times \text{area BOA}} \times \frac{\text{area BOA}}{V}$, or $ET = \frac{t^3 s}{12V}$. Let GE be denoted by d; then $ER = ds$, and RT, or the measure of the vessel's stability †$GZ = \frac{t^3 s}{12V} - ds$.

* Proposition and observations in pages 213, 214.

† In this expression for the measure of stability, s is the sine of the angle of the vessel's inclination, whatever be its magnitude: this value, for the stability of vessels which have a circular form, is the same with that which M. Bouguer gives for vessels of any form, when the angles of inclination are evanescent, the breadths at the water-line being $= t$, and the other conditions the same; from which circumstance, the following remarkable conclusion is inferred: if the measure of stability should be calculated for finite angles of inclination, by the rule M. Bouguer has given for the

Let $t = 100$, $s = \text{sin. } 15°$ to rad. $= 1$. $d = 13$, $V = 3600$. According to these conditions, $GZ = 2.63$. If, therefore, the vessel's weight should be 1000 tons, the stability will be equivalent to the weight of 1000 tons, acting to turn the vessel at the distance of 2.63 from the axis passing through G, or equivalent to a weight of 52.5 tons, acting at a distance of 50 from the axis.

CASE XI.

The vertical sections of a vessel are terminated by the arcs of a conic parabola.

Let the parabola BLA (fig. 20.) represent a vertical section of a vessel, floating with the axis DL perpendicular to BA, which coincides with the water's surface. G is the centre of gravity of the vessel. Suppose a ship, so formed, to be inclined from the upright through a given angle MOI. The breadth BA, and depth from the water-line, DL, being given, it is required to construct the measure of the vessel's stability.

The principal parameter being given from the conditions of the construction, from the vertex L set off LF, equal to a fourth part of the parameter: F is the focus of the parabola. In the line LF, take LI to LF, as the tangent of the given angle MOI to radius; and, in the line LI, take LX to LI, in the same proportion of the tangent of the angle MOI to radius. Through the point X, draw XV perpendicular to XL, intersecting the curve in the point V; set off LN equal to XL: join NV, which produce indefinitely, in the direction NVW;

angles of inclination that are evanescent, the stability of all vessels, at equal inclinations, thus calculated, whatever be their forms, would be the same as if the vertical sections were circular; the breadths at the water-line, position of the centres of gravity, and other elements, being the same.

NW is a tangent to the curve in the point V: through the point V, draw VK parallel and equal to DL; and, through the point K, draw CH parallel to NW: let DL be divided into five equal parts, and let LE be taken equal to three of those parts: make VQ equal to LE; and through Q draw $P\Gamma$ perpendicular to NW; through G draw GZ perpendicular to ΓP: GZ is the measure of the vessel's stability, when inclined from the upright through the given angle MOI. The demonstration follows. Through E, draw ET perpendicular to ΓP; and, through G, draw GR parallel to ΓP; let the parameter of the curve be denoted by p.

By the construction, LX : LI : : LI : LF : : tang. MOI to rad.
therefore - LX : LF : : tang.^{2} MOI : rad.^{2} and
and - LX : 4LF : : tang.^{2} MOI : 4rad.^{2}

By the properties of the curve,

LX : XV : : XV : 4LF
wherefore - LX : 4LF : : LX^2 : XV^2.
But - - LX : 4LF : : tang.^{2} MOI : 4rad.^{2}
therefore - LX^2: XV^2: : tang.^{2} MOI : 4rad.^{2}
and - - LX : XV : : tang. MOI : 2 rad.
or, since LX $= \frac{1}{2}$XN
$\frac{1}{2}$XN : XV : : tang. MOI : 2 rad.
or - - XN : XV : : tang. MOI : rad. but, by
the construction, XN : XV : : tang. XVN : rad.

consequently tang. XVN is equal to the tangent of MOI to the same radius; and therefore the angle XVN is equal to the angle MOI, or the given angle of the vessel's inclination from the upright. Moreover, since it appears from the construction, that the angle XVN is equal to the angle $N\Gamma P$, $N\Gamma P$ is equal to the vessel's inclination from the upright, and because the

line BA is parallel to XV, and the line CH parallel to NW, by the construction, it follows, that the angle ASH is equal to the angle XVN; wherefore the angle ASH is also equal to the angle MOI, or the given angle of inclination from the upright. VK being parallel to DL, and therefore a diameter of the curve to the point V, and CH being drawn parallel to NVW, which is a tangent to the curve in the point V, it follows, that VK bisects the line CH in the point K; KH therefore will be an ordinate to the diameter VK: and, since VK is by construction equal to DL, and DL, VK, are abscissæ of the segments BLA, CVH, respectively, it is known, from the properties of the figure, that the area of the segment BLA is equal to the area of the segment CVH; and consequently the area of the figure ASH will be equal to the area of the figure BSC. And since, when the vessel floats upright, the line AB coincides with the water's surface, and the area of the segment ALB is equal to the area of the segment CVH, it follows, that when the vessel is inclined from the perpendicular, through an angle ASH, equal to the given angle MOI, the surface of the water will intersect the vessel in the line CH. Moreover, since LE is to LD as 3 to 5, by the construction, and VQ is to VK in the same proportion of 3 to 5, by the properties of the figure, E is the centre of gravity of the area BLA, and Q is the centre of gravity of the area CVH, which represents the total volume displaced, when the vessel is inclined through an angle ASH, or MOI; and the line ΓQP being, by construction, drawn perpendicular to the water's surface CH, will be a vertical line passing through the centre of gravity Q of the volume displaced CVH: and GZ, drawn through the centre of gravity G, perpendicular to this line, will be the measure of

the vessel's stability, when inclined from the perpendicular through the given angle MOI.

From the preceding construction and demonstration, a property of stability is inferred, which may be expressed in the following proposition.

If the vertical sections of a vessel are terminated by the arcs of a conic parabola, and the sides of another vessel are parallel to the plane of the masts, both above and beneath the water-line, the stabilities of the two vessels will be equal at all equal inclinations from the upright, if the breadths at the water-line BA, and all the other conditions, are the same in both cases.

It is thus demonstrated:

For brevity, let the angle of inclination from the upright, or the angle ASH, be denoted by the letter S; let $BA = t$, and $LD = a$: rad. $= 1$.

From the preceding construction and demonstration, it appears that $XV : XN :: 1 : \text{tang. } S$, and by the properties of the figure $\frac{1}{2}XN : XV :: XV : p$, joining these ratios - $1 : 2 :: XV : p \times \text{tang. } S$.

Wherefore $XV = \frac{p \times \text{tang. } S}{2}$,

and $XL = \frac{XV^2}{p} = \frac{p \times \text{tang}^2 S}{4} = LN$;

also, since $XV : NV :: \text{cos. } S : 1$,

$$NV = \frac{XV}{\text{cos. } S} = \frac{p \times \text{tang. } S}{2 \times \text{cos. } S};$$

and because $LD = VK = a$, and $LE = VQ = \frac{3a}{5}$,

and the angle $VQP = ASH = MOI$, it follows that

$VP = \frac{3a \times \text{sin. } S}{5}$; and therefore

$$NP = NV + VP = \frac{p \times \text{tang. } S}{2 \times \text{cos. } S} + \frac{3a \times \text{sin. } S}{5};$$

therefore, $\Gamma N = \frac{NP}{\sin. S} = \frac{p}{2 \times \cos.^2 S} + \frac{3a}{5} = \frac{p \times \sec.^2 S}{2} + \frac{3a}{5}$,

and $L\Gamma = \Gamma N - LN = \frac{p \times \sec.^2 S}{2} + \frac{3a}{5} - \frac{p \times \text{tang.}^2 S}{4}$:

and, since $LE = \frac{3a}{5}$,

$$\Gamma E = \frac{p \times \sec.^2 S}{2} - \frac{p \times \text{tang.}^2 S}{4},$$

or $\Gamma E = \frac{p}{4} \times \overline{\sec.^2 S + 1}$, and, since the angle

$E\Gamma T = S$, $ET = \frac{p \times \sin. S}{4} \times \overline{\sec.^2 S + 1}$,

or $ET = \frac{p \times \text{tang.} S}{4} \times \overline{\cos. S + \sec. S}$.

This is the value of the line ET, when the area representing the volume immersed is terminated throughout by the parabolic arc, the said area being $= \frac{t^3}{6p}$ or $\frac{2}{3} \times BA \times DL$; but, if that form should extend to the sides AH, BC, only, the remaining part of the volume immersed being of any other figure,* and this entire volume should be of any magnitude V, the value of ET corresponding will be $\frac{p \times \text{tang.} S}{4} \times \frac{t^3}{6pV} \times \overline{\cos. S + \sec. S}$, or $ET = \frac{t^3 \times \text{tang.} S}{24V} \times \overline{\cos. S + \sec. S}$. And, since $ER = d \times \sin. S$, TR, or the measure of the vessel's stability, $GZ = \frac{t^3 \times \text{tang.} S}{24V} \times \overline{\cos. S + \sec. S} - d \times \sin. S$; precisely the same quantity which measures the stability at the angle of inclination S,† when the sides are parallel to the masts above and beneath the water-line: a coincidence not a little remarkable, and such as would not probably have been supposed to exist, except from the evidence of demonstration.

From this proposition it is inferred, that if the sides of a

* See pages 213, 214.

† Case 1. page 216.

vessel coincide with the arcs of the conic parabola, and the sides of another vessel coincide with the arcs of another conic parabola, whatever be the form thereof, varying according to the parameter, the weights of the vessels, breadths at the water-line, and the other conditions being the same in both cases, the stabilities of the two vessels, at all equal angles of inclination, will be equal. If, for instance, the forms of two vessels should be such as are represented in Tab. XII. fig. 21. and fig. 22. the weights and other conditions being the same, the stabilities of each of these vessels will be equal to that of a vessel PBQFAK, the sides of which are plane surfaces, parallel to the masts.

The propositions immediately preceding, relate to the conic or Apollonian parabola: they have been inserted, with a view of establishing and extending the theory of stability. It may also be remarked, that the sides of vessels are in some instances constructed nearly of these forms; for the same reasons, it may be not altogether useless, to examine on what principle the stability of vessels is to be investigated, when the forms of the sections are parabolic curves of the higher orders, such as are represented in fig. 23. The line *c*BCO is a conic or Apollonian parabola, *d*BDO is a cubic, and *e*BEO a biquadratic parabola. *f*BFO (fig. 23.) is a parabola of 8 dimensions, and *g*BGO a parabola of 50 dimensions, which are drawn from a geometrical scale, in order to give a true representation of the forms of these curves.

The general equation, determining the relation between the abscissæ and ordinates of any parabola, of the dimensions n, is $y^n = p^{n-1} \times x$, if the ordinates are drawn perpendicular to the axis of the curve; and $y = a + px + qx^2 + rx^3$, &c. $+ vx^n$.

if the ordinates are drawn parallel to the axis; y and x signifying the ordinate and corresponding abscissa; the other letters denoting constant or invariable quantities, to be determined by the properties of the figures. In these figures, it is observable, that the breadths toward the vertex O, are always greater in the curves which are of the higher dimensions; and, as the dimensions are continually increased, the figure approaches more nearly to a rectangular parallelogram,* with which it

* The radius of curvature of the conic parabola at the vertex (fig. 23.) is half the principal parameter; but, in all the parabolas of the higher orders, the radius of curvature at the vertex is infinite. Suppose x to represent the abscissa, or distance of the ordinate y from the vertex, measured along the axis of the curve: as x increases from o, the radius of curvature decreases till it becomes a minimum, and then increases: a difficulty seems to arise respecting the magnitude and variation of the radius of curvature, when, the dimensions being increased *sine limite*, the form of the curve approaches continually, and ultimately coincides with, the rectangular parallelogram. If the equation of the curve be $y^n = p^{n-1} \times x$, where p represents the parameter, the radius of curvature of the curve at the extremity of an ordinate y, of which the abscissa is x, will be found $= p \times \dfrac{\left(n^2 \times x^{\frac{2n-2}{n}} + p^{\frac{2n-2}{n}}\right)^{\frac{3}{2}}}{n \times \overline{n-1} \times p^{\frac{n-1}{n}} \times x^{\frac{n-2}{n}}}$, which quantity is a minimum when $x = p \times \left(\dfrac{n-2}{2n^3 - n^2}\right)^{\frac{n}{2n-2}}$: consequently, the least radius of curvature itself, or $r = p \times \dfrac{\left(\frac{3n-3}{n-2}\right)^{\frac{3}{2}}}{\frac{n-1}{n^2} \times \left(\frac{2n^3 - n^2}{n-2}\right)^{\frac{2n-1}{2n-2}}}$; and, when n is increased *sine limite*, the abscissa corresponding to the least radius of curvature, or $x = p \times \dfrac{1}{\sqrt{2} \times n}$, and the least radius itself, or $r = p \times \dfrac{\sqrt{27}}{2n}$; both of which quantities are evanescent, shewing that if the dimensions of the parabola are increased *sine limite*, the curvature at the extremity of the ordinate, when the abscissa $=$ o, is infinite, the radius of curvature being nothing, as it ought to be, at the point H of the parallelogram BHOD,

ultimately coincides, when the dimensions are increased *sine limite.* This extreme case has relation to the subject of stability: for, whatever may be the effect of giving to the sides of ships the forms of the several higher orders of parabolas, it is certain, that as the dimensions of these curves are increased, the stability will approach to that which is the consequence of making the sides parallel to the masts; but it has been shewn, that when the sides coincide with the form of a conical para-

considered as a parabolic curve of infinite dimensions, the two portions of the curve BH, HO, (fig. 23.) being inclined at a right angle, when coincident with the sides of a rectangular parallelogram: but, since the curvature is nothing at the vertex O, the abscissa being then $= 0$, and before the abscissa has increased to any finite line, the curvature at the extremity of the corresponding ordinate OH is infinite; and since the curvature between the points O and H must necessarily pass through all the intermediate gradations of magnitude, it becomes a question to define the abscissa and corresponding ordinate, when the radius of curvature is a finite line: 2dly, when it becomes evanescent; and, lastly, when it is again infinitely great. By referring to the preceding expressions for the abscissa and corresponding radius of curvature, it is found, that if p represents the parameter, and x is made $= \frac{p}{n^3}$, (the number n denoting the dimensions of the curve,) when n is increased *sine limite,* the radius of curvature will be greater than any line that can be assigned: and such is the curvature of any portion of the line OH, between the points O and H. 2dly, if x is $= \frac{p}{n^2}$, the radius of curvature will be $= p$, the ordinate approximating to equality with the line OH. 3dly, if $x = \frac{p}{n}$, the radius of curvature will be smaller than any finite line: and, lastly, if $x = p$, or any finite line, the radius of curvature will be greater than any assignable line: which conclusions are immediately inferred from the equation expressing the radius of curvature, or $r = p \times \frac{\overline{n^2 \times x^{\frac{2n-2}{n}} + p^{\frac{2n-2}{n}}}\big|^{\frac{3}{2}}}{n \times \overline{n-1} \times p^{\frac{n-1}{n}} \times x^{\frac{n-2}{n}}}$, when the number of dimensions n is increased *sine limite,* these successive changes in the radius of curvature taking place while the abscissa x is increased from 0 to any finite magnitude.

bola*, the stability† is the same as when the sides are plane surfaces, parallel to the plane of the masts. It is inferred that if the sides of a vessel are formed to coincide with a parabola of the lowest, and the sides of another vessel with a parabolic curve of the highest dimension, all the other conditions being the same, the stabilities of the two vessels will be equal in these two extreme cases.

In proceeding to ascertain the stability of vessels, the vertical sections of which coincide with any parabolic curve, the rigid strictness of geometrical inference cannot be well preserved, when the oblique segments are objects of consideration, on account of the complicated properties of the figures.‡ But, in these and similar cases, methods of approximation may be employed, by which the stability corresponding to any given figure of the sides may be inferred, to a degree of exactness exceeding any that can be necessary in practice. These methods of approximation are either such as are required for the mensuration of curvilinear areas, or geometrical constructions which exhibit the lineal measures of stability not strictly and rigidly true, but approaching, as nearly as may be desired, to the true and correct measures.

The methods of approximation to be used for the quadrature of curvilinear spaces, are founded on Sir ISAAC NEWTON's discovery of a theorem, by which, from having given any

* Case XI. pages 255, 256.

† The comparative stability, in this and similar observations, is understood to imply, that the vessels are inclined at equal angles of inclination from the upright, all the other conditions (the shape of the sides excepted) being the same.

‡ The areas of any parabolic segments, either direct or oblique, are geometrically quadrable, but, in the oblique segments, the positions of the centres of gravity are not determinable generally by direct methods.

number of points situated in the same plane, he could ascertain the equation to the curve which would pass through them all: and, by means of this equation, was enabled to express the ordinate in the curve, corresponding to an abscissa of any given length, as well as the area intercepted between any two of the ordinates. This discovery the author himself considered amongst his happiest inventions. Amongst the various uses of this theorem, that of determining by approximation the areas of curvilinear spaces is not the least considerable: for, by this means, the fluents of fluxional quantities, not discoverable by any known rules of direct investigation, are found, to a degree of exactness fully sufficient for any practical purpose, and with very little trouble of computation.

Mr. STIRLING, in his treatise intitled *Methodus differentialis*, has inserted a table for measuring curvilinear spaces terminated by parabolic curves, from having given 3, 5, 7, or 9 equidistant ordinates, and the abscissæ on which they are erected. The measures of the areas thus obtained are, under certain conditions hereafter stated, not approximations, but geometrically and strictly correct: the approximate values of curvilinear spaces, in general, are obtained from finding the correct areas terminated by parabolic lines which nearly coincide with the said curves, by passing through the extremities of the same ordinates.

The subjoined table contains Mr. STIRLING's rules for expressing the areas of curvilinear spaces, from the conditions which have been mentioned; also additional rules for measuring the areas which are included between the extremes of 2, 4, 6, or 8 equidistant ordinates: the whole of this table has been recomputed and verified.

TABLE OF AREAS.

Number of equidistant ordinates.	Areas.
2	$\frac{A}{2} \times R$
3	$\frac{A + 4B}{6} \times R$
4	$\frac{A + 3B}{8} \times R$
5	$\frac{7A + 32B + 12C}{90} \times R$
6	$\frac{19A + 75B + 50C}{288} \times R$
7	$\frac{41A + 216B + 27C + 272D}{840} \times R$
8	$\frac{36799A + 175273B + 64827C + 146461D}{846720} \times R$
9	$\frac{989A + 5888B - 928C + 10496D - 4540E}{28350} \times R$

In this table, the letter A denotes the sum of the first and last ordinate of the number opposite to it in the first column: B is the sum of the second and last but one: C is the sum of the third and last but two, and so on. The extreme letter, suppose D, (as in the rule opposite 8 ordinates,) is the sum of the two middle ordinates, if the number of ordinates is even; or the extreme letter, suppose D, (as in the rule opposite 7 ordinates,) is the middle ordinate alone, if the number of ordinates is odd. R is the entire length of the abscissa, which is always equal to the common interval between the ordinates, multiplied by the number of ordinates diminished by unity.

Let the area to be measured be terminated by the curve line ABCD, &c. (Tab. XIII. fig. 24.): A′I′ is an abscissa, on which a number of equidistant ordinates AA′, BB′, CC′, &c. are erected at right angles. If ABCD, &c. represents a parabolic line of any dimension, suppose n, the relation between the ordinates and ab-

scissæ being expressed by the equation $y = a + px + qx^2 + tx^3$, &c. $+ ux^n$, (in which case, the ordinates are drawn parallel to the axis of the curve,) a measure of the area contained between the extremes of $n + 1$ ordinates will be obtained with geometrical exactness, by computing from the rule in the table which is opposite the number of ordinates $n + 1$, supposing the table to extend to that number: but if, as it usually happens in cases which practically occur, that the nature of the curve is unknown, or the conditions in other respects different from those which are required for the mensuration of the area with perfect correctness, it becomes a question, which particular rule in the table should be adopted for inferring an approximate value of the area, since an exact quadrature is not obtainable. For this purpose, there are several reasons for preferring the rules opposite the number of ordinates 2, 3, and 4 to the others, which require a greater number of ordinates; the common distance between them being the same. In the first place, the rules here pointed out are far less troublesome in the application; a circumstance which ought to have weight, although of less importance than another consideration, which is, that the results derived from these rules, particularly from the two latter, will in general approximate as nearly to the true value, sometimes more nearly, than those which are obtained by calculating from the other more complicated theorems, unless the curve should happen to be such as admits of being correctly measured by any of the rules requiring a greater number of ordinates; a circumstance not likely to occur in practical mensurations.

Let it be proposed to measure by approximation any curvilinear space AA′I′IA (fig. 24.) For brevity, let the successive ordinates AA′, BB′, CC′, &c. be denoted by the letters a, b, c,

&c. respectively; also, let the common distance between the ordinates (fig. 23.) or $A'B' = B'C' = C'D'$ be $= r$: according to the theorem for measuring the area contained between two ordinates, or $\frac{A}{2} \times R$, the curve line AB is supposed to coincide with the right line AB which joins the extremities of it; the space measured by this rule is the trapezium AA'B'BA; and, since $A = a + b$, and $R = r$, the area of the trapezium, or $\frac{A}{2} \times R = \overline{a + b} \times \frac{r}{2}$.

According to the rule opposite 3 ordinates, the curve line ABC is supposed to coincide with a portion of the conic parabola, the axis of which is parallel to the ordinates. And since, by this rule, the area $= \overline{A + 4B} \times \frac{R}{6}$, in which expression $A = a + c$, $B = b$, and $R = 2r$; by substituting these values, the area $AA'C'CA = \overline{a + 4b + c} \times \frac{r}{3}$. If the curve ABC should actually be a portion of the conic parabola, the given ordinates being parallel to the axis of the curve, the area AA'C'CA will be measured by this rule with exactness absolutely perfect; and, the more nearly the curve which terminates the area approaches to the form of the conic parabola, the more nearly will the result of calculating by this rule approximate to the true value of the area. But it is evident, that since in this approximation, the arc of a conic parabola is drawn through the points A, B, C, being the same points which terminate the ordinates of the given curve, the difference between the parabolic area and that which is given must, in most (except extreme) cases, be next to an insensible quantity, when applied to practical mensuration.

In the mensuration of areas by the rule opposite 4 ordinates,

a parabolic curve line is supposed to be drawn through the points A, B, C, D, of the 3d dimension, such as the arc of a cubic parabola, the ordinates of which are parallel to the axis of the curve, and the area terminated by this curve line is assumed to approximate to the given area AA′D′DA: by this rule, the area $= \overline{A + 3B} \times \frac{R}{8}$, in which expression $A = a + d$, $B = b + c$, and $R = 3r$, which being substituted for their respective values, the area AA′D′DA $= \overline{a + 3b + 3c + d} \times \frac{3r}{8}$.

In order to bring these rules into a form convenient for practical use, let it be proposed to measure the area AA′G′GA (fig. 24.) intercepted between the extremes of 7 ordinates.

1st. Suppose the right lines AB, BC, CD, *&c.* to be assumed, instead of the curve lines AB, BC, CD, *&c.* as terminations of the space to be measured: then the area AA′G′GA will be equal to the sum of six trapeziums; AA′B′B, BB′C′C, CC′D′D and so on.

The area of the trapezium AA′B′B $= \overline{a + b} \times \frac{r}{2}$, by the rule opposite 2 ordinates: by the same rule, the area of the trapezium BB′C′C $= \overline{b + c} \times \frac{r}{2}$; the area of the trapezium CC′D′D $= \overline{c + d} \times \frac{r}{2}$, and so on. By adding these six separate areas, the sum will be the area of the space AA′G′GA $= \overline{a + 2b + 2c + 2d + 2e + 2f + g} \times \frac{r}{2}$. The law of continuation for a greater number of ordinates is obvious. This rule is precisely the same with that which is given by M. Bouguer, in his work entitled "*Traité du Navire*,"* under a form somewhat different: his rule is this; from the sum of all the ordinates subtract $\frac{1}{2}$ of the sum of the first and last; the

* Page 112.

result multiplied into the common distance between the ordinates will be the exact area of the figure, considered as consisting of trapezia, and an approximate value of the curvilinear area in which the said trapezia are inscribed. This rule he professes not to be a very correct approximation, but such as may be deemed sufficient for most practical mensurations. It must be acknowledged, that in mensurations independent of others, the errors arising from this rule are often not considerable, (in many cases they are very small;) but, considering that in naval mensurations, areas obtained by approximation are necessarily the data from which other results are to be inferred, also by approximation, a doubt may arise whether the errors thus accumulated may not, in some cases, become too great; at least it may not be improper to be provided with rules which may be relied on, as approximating more nearly to the true measures of areas.

Let the same area be measured by the rule opposite 3 ordinates, according to which it is supposed that the curve line ABC coincides with the arc of the conic parabola. By this rule, the area $AA'C'CA = \overline{a + 4b + c} \times \frac{r}{3}$: also the area $CC'E'EC = \overline{c + 4d + e} \times \frac{r}{3}$; and the area $EE'G'GE = \overline{e + 4f + g} \times \frac{r}{3}$: adding these three areas together, the sum is the area $AA'G'GA = \overline{a + 4b + 2c + 4d + 2e + 4f + g} \times \frac{r}{3}$.

This rule is the same with that which Mr. Simpson has demonstrated in his Essays, page 109, from the properties of the conic parabola, perhaps not noticing that it was to be found in Mr. Stirling's table of areas.

Mr. CHAPMAN, an eminent author on the subject of naval architecture,* applies this theorem to naval mensurations, as a substitute, and certainly an useful one, to the less perfect rules which are employed for this purpose, in the works of M. BOUGUER and other authors. The example by which Mr. CHAPMAN illustrates the use of this rule is the same with that which is given in Mr. SIMPSON's Essays.

This approximation to the measures of areas being applicable

* The following observation on this theorem is inserted in the Report from the Committee of the French Royal Marine Academy, who were appointed to examine the translation of Mr. CHAPMAN's Treatise on Naval Architecture, by M. VIAL DE CLAIRBOIS; this report is prefixed to the French edition of Mr. CHAPMAN's work.

"Ce célèbre constructeur commence par donner une nouvelle méthode de calcul de "déplacement, qui sans être beaucoup plus longue que celle que l'on emploie com-"munément, donne un résultat infiniment plus exact. On considére ordinairement "les parties curvilignes des plans de flottaisons, ou de gabarits, entre les extrémités "des ordonnées, comme des droites; M. CHAPMAN les regarde comme des parties "paraboliques; et, de la nature de cette section conique, et du trapeze, il tire une "expression sur laquelle il fonde un calcul assez simple," *&c.*

A comparison of the results derived from this rule, and from that which is employed by M. BOUGUER, does not seem to confirm the opinion of the very superior exactness which the committee here attribute to the former rule: that it is more exact there is no doubt, especially when the curvature is at all irregular in respect to its variation, and the results inferred are data on which other computations are to be founded; but, in many of the cases which occur in practical mensurations, the latter rule approximates to the required results sufficiently near the truth, as will appear by the instances in the subsequent pages. The expression "une nouvelle méthode" cannot be understood to mean a rule of computation newly invented, but one which Mr. CHAPMAN has first applied to naval mensurations. In this sense, the theorem inserted in the 265th and 268th pages of these papers would be entitled to the appellation of "a new method:" but it has already been shewn, that the three rules here described, and employed in the computations which follow, are only particular cases of the general method demonstrated in the works of Sir I. NEWTON, STIRLING, SIMPSON, and other authors.

only when the number of given equidistant ordinates is odd, to obtain the area when the number of ordinates is even, another rule, to be employed either singly, or in conjunction with the former, may be selected from Mr. STIRLING's table of areas. It is that which stands opposite 4 ordinates.

Let it be proposed to measure the area AA′G′GA. According to this rule, the area AA′D′DA $= \overline{a + 3b + 3c + d} \times \frac{3r}{8}$

also the area DD′G′GA $= \overline{d + 3e + 3f + g} \times \frac{3r}{8}$

these two areas being added together; the sum will be the area AA′G′GA $= \overline{a + 3b + 3c + 2d + 3e + 3f + g} \times \frac{3r}{8}$.

When the number of given equidistant ordinates is small, these theorems will be most conveniently used in the forms here given; but, when the ordinates are numerous, the trouble of arithmetical computation will be considerably abridged, by employing them according to the general rules inserted underneath.

These theorems for approximating to the values of areas, may be applied, with advantage, to the integration of fluxional quantities, the fluents of which cannot be obtained by direct methods; or, if obtained, requiring very long and troublesome calculations.*

Suppose z to represent the abscissa of a curve, on which the or-

* On this principle, the rules of approximation here given are applicable to determine the positions of the centres of gravity, both of areas and solid spaces. If y is put to represent the ordinate erected perpendicular to an abscissa, at the distance z from the initial point thereof, the fluent of $yz\dot{z}$ (fig. 24.) will be the sum of the products arising from multiplying each ordinate into the small increment $\dot{z}$, and also into the distance z from the initial point. And, since the area intercepted between the ordinates AA′ and y is the fluent of $y\dot{z}$, it follows, that the distance of the centre of gravity of this curvilinear space from the ordinate AA′, measured on the abscissa A′I′, is $= \frac{\text{fluent } yz\dot{z}}{\text{fluent } y\dot{z}}$. The approximate values of these fluents are obtained from the Rules I. II.

dinates (expressed by Z, a general term or function of z), a, b, c, d, *&c.* are erected at right angles, and at intervals each of which is $= r$, so that when $z = o$, $Z = a$: when $z = r$, $Z = b$: when $z = 2r$, $Z = c$, and so on. If innumerable ordinates or values of Z be supposed drawn between each of those which are given, at the common very small interval $\dot{z}$, the sum of the products arising from multiplying each of the ordinates into the increment $\dot{z}$, that is, the fluent of $Z\dot{z}$, will be found, by approximation, according to the three following rules; which may be not improperly termed, rules for approximating to the integral values of fluxional quantities. According to

RULE I.

$$\text{Fluent of } Z\dot{z} = \overline{P - \tfrac{S}{2}} \times r;$$

in which expression,

$P =$ the sum of all the ordinates $a + b + c + d$, &c.
$S =$ the sum of the first and last ordinate.
$r =$ the common distance between the ordinates.

RULE II.

$$\text{Fluent of } Z\dot{z} = \overline{S + 4P + 2Q} \times \tfrac{r}{3};$$

in which expression,

$S =$ the sum of the first and last ordinate.
$P =$ the sum of the 2d, 4th, 6th, 8th, *&c.* ordinate.
$Q =$ the sum of the 3d, 5th, 7th, 9th, *&c.* ordinate, (the last excepted.)
$r =$ the common distance between the ordinates.

and III.; and the positions of the centres of gravity are thus determined according to the methods of computation employed in the subsequent pages. The position of the centre of gravity in solid bodies, is determined by a similar application of these rules.

RULE III.

$$\text{Fluent of } Z\dot{z} = \overline{S + 2P + 3Q} \times \frac{3r}{8};$$

in which expression,

S = the sum of the first and last ordinate.

P = the sum of the 4th, 7th, 10th, 13th, &c. ordinate, (the last excepted.)

Q = the sum of the 2d, 3d, 5th, 6th, 8th, 9th, &c. ordinate.

r = the common distance between the ordinates.

It is to be observed, that the first of these rules approximates to the fluent, whatever be the number of given ordinates. The second rule only requires that the number of ordinates shall be odd. To apply the third rule, it is necessary that the number of ordinates given shall be some number in the progression 4, 7, 10, 13, &c. that is, the number of ordinates must be a multiple of 3 increased by unity. But, in every case, the approximate fluent may be obtained, either from the Rule II. or the Rule III. or by employing both rules conjointly.

Before these theorems are applied to practical mensurations in naval architecture, it may be satisfactory to examine, by a few trials, to what degree of exactness they approximate to the correct values of curvilinear spaces. This will be known, if the area of some curve, which is exactly quadrable by other geometrical rules, be measured by them. Such as a parabolic figure of which the equation is $y^8 = p^7 x$, x being the abscissa coincident with the axis, and y the corresponding ordinate perpendicular to it.

The semi-area of this parabola (fig. 25, 26.) is known to be $xy \times \frac{8}{9}$; and the curve is termed a parabola of 8 dimensions.

OD is an abscissa, being a portion of the axis of this curve, and the parameter is assumed = OD. If, therefore, an ordinate BD, or DA, is drawn thr ugh the point D perpendicular to DO; the lines DB, DO, and DA, will all be equal. Let BD be divided into 10 equal parts,* considered in this instance as an abscissa, on which, at the points of division, the several ordinates are erected perpendicular to BD, denoted in the figure by the letters *a*, *b*, *c*, *d*, &c. If BA is assumed = 100 equal parts, DB, DO, and DA, are each = 50, and the common interval between the ordinates = 5; the numerical values of the successive ordinates *a*, *b*, *c*, &c. are expressed in the annexed table.

a	= 50.0000
b	= 50.0000
c	= 49.9999
d	= 49.9967
e	= 49.9672
f	= 49.8047
g	= 49.1602
h	= 47.1175
i	= 41.6113
k	= 28.4766
l	= 0.0000
Sum of all the ordinates	= 466.1341

According to the Rule 1. making the sum of all the ordinates, or P = 466.1341, the sum of the first and last ordinate, or S = 50 : $r = 5$ = the common distance of the ordinates.

The area BDO $= \overline{P - \frac{S}{2}} \times r$ = 2205.670

correct area $= \frac{50 \times 50 \times 8}{9}$ = 2222.222

Difference † or error of the approximation = 16.552

* Any line being assumed in a curve as an abscissa, lines drawn parallel to each other, and intercepted between the abscissa and the curve, are termed ordinates. To exemplify these rules for approximating to the areas of curvilinear spaces, it was necessary to consider the ordinates as being drawn in some cases parallel, and in others perpendicular, to the axis.

† It must not be concluded, from this instance, that the errors in measuring curvilinear areas by the Rule 1. will be usually so great as 16 parts in 2222. In applying the Rule 1. to this parabolic space, the quick variation of curvature in some parts of the curve, causes the space so measured to deviate more from the truth than would happen in ordinary cases, such as commonly occur in practical subjects. If, instead of

If the same area is measured by the Rule II. the process will be as underneath:

Sum of all the ordinates -		= 466.1341
Sum of the first and last, or S =	50.0000	
Sum of the 2d, 4th, 6th, &c. or P =	225·3955	
S + P =	275·3955	275·3955
Sum of the 3d, 5th, 7th, &c. (except the last) or Q		= 190.7386

and r being $= 5$, the area BDO $= \overline{S+4P+2Q} \times \frac{r}{3} =$ 2221.765

Correct area - = 2222.222

Difference or error of the approximation = ·457

Let the same area be measured by the Rule III. the area DOKI, between the two ordinates a and b, being $= 5 \times 50 = 250$, the remaining area, from the ordinate b to the ordinate $l = 0$, will be obtained from the following computation:

The sum of all the ordinates		= 416.1341
Sum of the first and last, reckoning b the first, and the last $l = 0$ or S	= 50.0000	
Sum of the 4th, 7th, 10th, &c. (b being the 1st.) or P - -	= 97·0847	147·0847
Sum of the 2d, 3d, 5th, 6th, &c. from b, or Q		= 269·0494

the total area = DBO, (fig. 25.) a portion of it, which is contained between the ordinates a and g, should be measured, the area, computed from either of the three rules, would deviate very little from the truth, as appears from the following results.

Areas contained between the ordinates a and g,

computed by	Rule I.	Rule II.	Rule III.
Areas	1496.74	1497·17	1497·13
Correct area	1497·20	1497.20	1497.20
Difference or error of approximation -	·46	·03	·07

If the rule in the table of areas opposite 7 ordinates should be applied to measure the area between the ordinates a and g, (fig. 25.) the result would be geometrically correct.

And, since $r = 5$, the area between the ordinates

b and $l = \overline{S + 2P + 3Q} \times \frac{3r}{8}$ -	$= 1971.220$
Area IKOD between the ordinates a and b	$= 250.000$
area BDO - -	$= 2221.220$
Correct area BDO - -	$= 2222.222$
Difference or error of the approximation	$= 1.002$

In applying these rules, it is necessary to observe, that if the ordinates are drawn perpendicular to the axis of the curve, whenever the area to be measured, or any part of it, is adjacent to the vertex O, the area found by these rules will be the least exact: in such cases, it will be requisite to assume an abscissa near the vertex O, perpendicular to the axis: by erecting equidistant ordinates upon it, parallel to the axis, the area will be found, with the same exactness as in the other cases, which will appear by the following computations.

DOA (fig. 26.) is a semiparabola, similar and equal to DOB.

Let the line DO = 50 be divided into 10 equal parts, each = 5; and, through the points of division, let the successive ordinates b, c, d, &c. be drawn perpendicular to DO: according to the preceding observations, if the entire area DOA should be computed by either of the three rules, the result would be less exact than in the former cases. To obtain an approximate value of the area, sufficiently near the truth, a portion of the area adjacent to the vertex O, suppose XEO, is to be separately computed. If OX be = 10, the line XE will = 40.8890, which being divided into six equal parts, each of them will = 6.815: let the ordinates p, q, s, t, *&c.* be erected at the points of division, perpendicular to XE:

p	=	10.0000
q	=	10.0000
s	=	9.9985
t	=	9.9805
u	=	7.6750
v	=	9.6100
w	=	0.0000
	=	57.2640

From these ordinates, the area OXE is found by the Rule II. to be $159.8390 \times \frac{6.815}{3} = 363.101$.

For obtaining the area DAXE, the ordinates *a*, *b*, *c*, *d*, &c. erected on the abscissa DX, are as expressed underneath:

a	=	50.000
b	=	49.346
c	=	48.624
d	=	47.820
e	=	46.907
f	=	45.851
g	=	44.590
h	=	43.014
i	=	40.889
Sum of all the ordinates	=	417.041
k	=	37.495
l	=	0.000

The area DAXE, between the ordinates *a* and *i*, is found by the Rule II. to be - -	=	1858.760
The area between the ordinate *i* and *l*, before found	=	363.101
*Entire area DOA	=	2221.861
Correct area -	=	2222.222
Difference or error of the approximation -	=	.361

* If the area between the ordinate *a* and *i* had been computed by the Rule I. the result would have been nearly the same.

Area between the ordinates *a* and *i*, by Rule I, is -	1857.98
Area between the ordinates *i* and *l*, before found, is	363.10
Entire area DOA - -	= 2221.08
Correct area - - -	= 2222.22
Difference or error of the approximation - -	1.14

If the total area DOA (fig. 26.) should be measured by one computation, suppose from Rule II. the

Area would be found - -	= 2176.875
Correct area - - - -	= 2222.222
Difference or error of the approximation -	= 45.347

If this area should be computed according to the Rule I. the error of the approximation will be found - - - - - = 74.54

It is easily shewn, that these errors, which are far from inconsiderable, arise almost wholly from the mensuration of the area OXE, (fig. 26.) adjacent to the vertex O. For, by measuring that area, according to the Rule II. from three ordinates $l = 0$, $k = 37.495$, $i = 40.889$, the area is found to be 318.115
Whereas the correct area OXE $= 10 \times 40.889 \times \frac{8}{9} =$ 363.457

Difference or error from computing the area OXE by Rule II. - - - - = 45.342
Scarcely differing from - - = 45.347
which was found to be the error from computing the entire area DOA by this rule.

If the area between the ordinates *a* and *k* be measured by the Rule III. it is found to be - - = 2055.390
Area between the ordinate *k* and *l*, by the properties of the figure, is $= 37.495 \times 5 \times \frac{8}{9}$ - = 166.640

Entire area DOA -	= 2222.030
Correct area - -	= 2222.222
Difference or error of this approximation	= .192

From these computations it is evident, that the rules here given, when employed with attention to the necessary limita-

tions and restrictions, will approximate to the measures of areas, to a degree of exactness fully sufficient for naval mensurations; and further, will be useful in determining by approximation the integral values of fluxional quantities in general, especially those which occur in the investigation of practical subjects.

CASE XII.

Still supposing the vertical sections of a vessel to be equal and similar figures, let BOA (fig. 27.) represent one of these sections; the figure being either a curve of the higher dimensions, or a curve not formed according to any geometrical law, of which the lengths of the ordinates, and of any other lines given in position, are supposed to be measurable, and given in quantity: the angle at which the vessel is inclined from the upright, and the other necessary conditions being known, it is required to find, by geometrical construction, a line which shall approximate nearly to the measure of the vessel's stability.

1st *Method.*

BA represents the intersection of the water's surface when the vessel floats upright: bisect BA in the point D; and, through D, draw the line NDM inclined to the line BA at an angle ADM, equal to the given angle of the vessel's inclination: let the area of the figure ADMb, also the area of the figure BDNc, be found by means of the rules which have been described: suppose the area ADMb to be greater than the area BDNc, and let E represent the difference between them: from

the point D, in the line DA, set off a line * $DS = \frac{E}{NM \times \sin. ADM}$: a line CSH, drawn through the point S, parallel to NM, will cut off the area ASHbA, very nearly equal to the area BSCcB; (fig. 27.) consequently, when the vessel is inclined to the given angle, the water's surface will intersect the vessel in the line CSH. (Tab. XIV. fig. 28.) Draw the lines AH, BC. Let M and I be the centres of gravity of the triangles ASH, BSC, respectively; through M and I, draw Ml, Ik, perpendicular to CH; through b, † the centre of gravity of the area AbH, draw bU perpendicular to SH; and, through c, the centre of gravity of the area BcC, draw cR perpendicular to CH: in the line lU, take lL to LU as the area AHb is to the area ASHb; and, in the line kR, take kK to kR as the area BcC is to the area BSCc. Let G be the centre of gravity of the vessel; and let E be the centre of gravity of the displaced volume when the vessel floats

* Let the area ASHb be supposed equal to the area BSCc, (fig. 27.) and make either of them $= A$. Let the space DMHS be denoted by M, and the space NDSC by N: then the area ADMb will approximate very nearly to the quantity $A + M$, and the area BDN to $A - N$. The difference of these areas will be $M + N$, which is equal to the area $NMHC = E = MN \times DY$; and, consequently, $DY = \frac{E}{MN}$; and, because $DY : DS :: \sin. DSY$, or ADM to radius, it will follow that $DS = \frac{E}{MN \times \sin. ADM}$.

† In these small curvilinear spaces, it will be sufficient to assume the positions of the centres of gravity by estimation, on a supposition that the curve coincides with the arc of a common parabola; in which case, the centre of gravity is situated at the distance of $\frac{2}{5}$ of the abscissa from the ordinate, or chord which joins the extremities of the curve. The position of the abscissa is determined by drawing chords parallel to the given chord, and by drawing a line through the points which bisect the several chords. But, when the curvilinear spaces AHb are extremely small, as represented in this figure, (fig. 28.) no sensible difference in the result will ensue, whether the line bU is drawn through the centre of gravity of this curvilinear space, or through any other point which is adjacent to that centre.

upright. Through the point E, draw EV parallel and equal to KL: and, in EV, take ET to EV as the area ASHb is to the area representing the entire volume displaced: through G, draw GU parallel to CH; and, through T, draw TZ perpendicular to GU, intersecting the line GU in the point Z. GZ is the measure of the vessel's stability.

2d Method.

Let BOA (fig. 29.) be the given vertical section of a vessel, intersected by the water's surface BA when floating upright. G is the vessel's centre of gravity: E is the centre of gravity of the volume displaced in the upright position. Let the area BOA be measured by either of the three Rules, suppose Rule 1.; and through D, the bisecting points of BA, draw NDM inclined to the line BA in the angle ADM, equal to the given inclination of the vessel from the upright. Let the area NOAM be measured, by erecting equidistant ordinates on the line MN. If the area, so found, is equal to the area BOA, the area DBN will be equal to the area ADM. But, if they are unequal, let the difference be represented by E, and from D, toward the largest of the areas, suppose ADM, set off DS $= \frac{E}{NM \times \sin. ADM}$; and, through S, draw CSH parallel to NM. The area ASHb will approximate to equality with the area BSCc; and, consequently, when the vessel is inclined through the given angle ASH, it will be intersected by the water's surface in the line CH. On the line HC, let the equidistant ordinates a, b, c, d, &c. be erected perpendicular to CH; and let the common interval between the ordinates be $= r$. Let the measure of the area CLFK be obtained, and let m be the centre of gravity of this area: through the point m, draw mP perpendicular to CH: let each of the successive

given ordinates be multiplied into its perpendicular distance from the ordinate a. The terms resulting will be $a \times 0$, $b \times r$, $c \times 2r$, $d \times 3r$, &c.; let these terms be added together, and half the sum of the first and last term being subtracted from the amount, let the result be denoted by the letter C; Cr — area CLFK × KP will be the sum of the products arising from multiplying each evanescent area QX into its distance QK from the first ordinate a. In the line KH, set off a line KI* $= \frac{Cr - \text{area CLFK} \times \text{KP}}{\text{area COAH}}$. Through the point I, draw IT perpendicular to CH; and, through the vessel's centre of gravity G, draw GZ perpendicular to IT. GZ is the measure of the vessel's stability, when it is inclined from the upright through the angle ASH.

In the cases which have preceded, the vertical sections of vessels, or segments of vessels, are assumed as equal and similar figures: whereas, in reality, the form and magnitude of the sections are gradually changed, according as they intersect the longer axis at a greater or less distance from the head or stern. The solutions of the preceding cases, and the principles therein established, may be next applied to investigate the stability of vessels, taking into consideration the form and magnitude of each particular section intersecting the longer axis at right angles, and at equal distances; taking into account also, by the methods which have been described, all the sections intermediate between those which are given, that may be conceived to intersect the axis at a very small common interval.

* Since the Rule I. is employed in obtaining the value of the quantity Cr, according to this computation, the area COAH ought to be measured by the same rule; in which case, the line KI will be determined nearly with the same exactness as by either of the Rules II. or III.

CASE XIII.

The longer axis of a vessel is supposed to be divided into a given number of equal parts, and vertical sections to pass through the several points of division, intersecting the axis at right angles: the form and magnitude of each particular section being given, with the common distance between them, the positions of the centres of gravity of the vessel, and of the volume displaced, and the distance of the water-section from the keel, being known, it is required to construct the measure of the vessel's stability, when it is inclined from the upright through a given angle.

Let QBOAW (Tab. XV. fig. 30.) represent any vertical section of a vessel; suppose it to be the greatest or principal section: BA is the breadth of this section at the water-line, when the vessel floats upright: let CH represent the line which coincides with the water's surface, when the vessel is inclined from the upright through the given angle ASH. From the nature of the conditions, it is sufficiently evident that the point S, in any individual section, will not be determined on the same principle by which the position of that point was fixed according to the former solutions; that is, by making the area ASH equal to the area BSC; because, the volume immersed, and that which is caused to emerge in consequence of the vessel's inclination, will not now be proportional to these areas, as they* are on a supposition that the vertical sections are similar and equal figures. But, in the present case, the vertical sections being different, both in form and magnitude, the water's sur-

* See page 210.

face, intersecting the vessel in a plane passing through the line CH, when the vessel is inclined, will so divide the areas of the several sections, that although the area ASH*b* may not be equal to the area BSC*c*, in any of the vertical sections, yet the volume immersed, corresponding to, and included between, the areas of the figures ASH*b*, taken from the head to the stern of the ship shall be equal to the emerged volume which is included between the areas BSC*c*, in the several sections. Suppose the breadth of any section at the water-line* to be denoted by BA, and to be bisected in the point D. A vertical plane passing through the vessel's longer axis and the centre of gravity G,† and dividing the ship into two parts perfectly similar and equal, will pass through the points D, in all the sections: this plane may be termed the plane of the masts. It is easily shewn, that at whatever distance DS, from the middle point D, the plane of the water's surface, passing through the lines CH, intersects the line DA in any one section, when the vessel is inclined through the angle ASH, it will intersect the line DA at the same distance from the middle point D in all the other sections; that is, the distance DS will be the same in all the sections: for, by the supposition, the vessel is inclined round the longer axis, and consequently, the intersection of the two planes, passing through the lines BA and CH, will be parallel

* The same letters which are used to denote the several lines, in this vertical section, must be understood to represent the lines similarly drawn in each of the other sections. In the present instance, BA does not represent the breadth at the water's surface, of the principal, or any other individual vertical section, but represents generally that breadth, in any of the sections that may be referred to.

† Represented by the point G projected on the plane BOA. The centre of gravity E is, in like manner, here represented by projection on the same plane.

to the longer axis, and therefore parallel* to a line drawn through all the points D, from one extremity of the vessel to the other; the several lines DS are the perpendicular distances of these parallel lines, and are consequently all equal. In the next place, it is requisite to determine the magnitude of the line DS, according to the given conditions: whatever be the position of the points S, if lines CH are drawn through S, in each of the sections, inclined at an angle to the line BA, equal to the given angle of the vessel's inclination, the same plane will pass through all the lines CH. It is required to ascertain at what distance DS, from the points D, the plane CH, coinciding with the water's surface when the vessel is inclined, must pass, so as to cut off a volume on the side ASH*b*, being the volume immersed, which shall be equal to the volume in the side BSC*c*, which has emerged from the water, in consequence of the vessel's inclination.

In each section, through the middle point D, draw a line NDW, inclined to BA at an angle ADW, equal to the given angle of the vessel's inclination; the same plane will pass through the lines NDW, in all the sections. By the methods which have been described, let the area of the figure ADW*b* be measured in each section; from these equidistant areas, the solid contents of the volume between the two planes DA† and DW, and the side of the vessel intercepted, may be inferred by

* The points *D* being coincident with the water's surface, a line passing through them must be horizontal; and being, by the supposition, situated in the same plane with the longer axis, must therefore be parallel to it.

† Since the same plane passes through the lines DA, drawn coincident with the water's surface in all the sections, this plane may be supposed projected into the line DA on the plane DOA. For similar reasons, the line DW represents the plane which passes through all the lines DW in all the sections.

computing according to the Rules II. and III.; suppose this volume to be denoted by the letter P, and the volume contained between the planes DB, DN, and the side of the vessel, found by similar operations, to be denoted by the letter Q. Let the area* of the section of the vessel passing through the lines NDW be measured, from having given the lines NW in all the sections from the head to the stern, and let this area be put = R. If the volumes P and Q should be unequal, P being the greatest, in the line DA, set off, in each section, a line DS $= \frac{P-Q}{R \times \sin. ADW}$. If a plane CSH be drawn passing through all the points S, and inclined to the plane BA at the given angle of the vessel's inclination, the solid contents of the volume between the planes SA, SH, and the intercepted side of the vessel, will approximate to equality with the volume contained between the planes SB, SC, and the intercepted side of the vessel. Since, therefore, the water's surface coincides with the plane BA when the vessel is upright, when it is inclined round the longer axis, through the given angle ASH, the water's surface will intersect the vessel in the direction of a plane passing through the lines CH, in all the sections.

Let the solid contents of the volume immersed, or emerged, by the inclination, be denoted by the letter A.

In the section QBOAW, let M be the centre of gravity of the triangle ASH, and let *b* be the centre of gravity of the curvilinear area AH*b*; also, let I be the centre of gravity of the triangle BSC, and let *c* be the centre of gravity of the curvilinear area BC*c*: through these points, draw the lines

* That is, the area of the section coinciding with the water's surface, when the vessel is inclined to the given angle.

M*l*, *b*U, I*k*, *c*R, perpendicular to CH; and, in the line *l*U, take a line *l*L, which is to *l*U as the curvilinear area AH*b* is to the area ASH*b*. Through the points S, in all the sections, let a line F*f* be drawn perpendicular to SH; the same plane will pass through all these lines. LS will be the distance of the centre of gravity of the area ASH*b* from the plane F*f*. The products arising from multiplying each area ASH*b* into the distance SL, of its centre of gravity, from the plane F*f*, are to be calculated in all the sections; from which products, by means of the Rules* I. II. and III. the sum of the products arising from multiplying each evanescent solid, of which the base is the area ASH*b*, and the thickness a small increment of the axis, into the distance SL of its centre of gravity from the plane F*f*, will be obtained. The sum of these products, divided by the solid contents of the volume immersed A, will be the distance of the centre of gravity of that volume from the vertical plane F*f*. Suppose this distance to be equal to the line SQ: let the distance PS, of the centre of gravity of the volume emerged, or BSC*c*, from the plane F*f*, be found from similar computations; the line PQ will be the distance of the centres of gravity of the volumes ASH*b*, BSC*c*, estimated in the direction of the line CH, perpendicular to the plane F*f*.

The solid contents of the entire volume displaced by the ship, are to be obtained from the areas, either of the vertical or horizontal sections.

* Whenever the Rules I. II. and III. are referred to, it is meant that the computation is to be made from one or more of these rules, according to the number of ordinates given, or as other circumstances may direct.

† See Appendix.

The ordinates drawn in the several sections being set down in regular order, the area of any horizontal section is to be found from the corresponding series of ordinates, by means of the Rules II. and III. and, by the same rules, from the areas of the horizontal sections so determined, the solid contents of the total volume immersed are to be inferred; some allowance being made for the irregular parts of the volume adjacent to the head and stern, if attention to these additional volumes should be thought necessary. That part of the volume which is contained between the keel and the nearest horizontal area, is obtained by first finding the area of each vertical section between the keel and nearest ordinate: from these areas, by means of the Rules II. and III. the solid contents of the volume between the keel and nearest horizontal section will be measured, and is to be added to the volume before found, which is contained between the two extreme horizontal sections.

Let the solid contents of the displaced volume be denoted by V.

From the areas of the horizontal sections, and the common interval between them, the distance DE, of the centre of gravity of the volume immersed, from the water-section, is to be obtained by means of the Rules II. and III.; by finding the sum of the products arising from multiplying each evanescent solid, of which the base is any horizontal section, and the thickness a small increment of the vertical axis, into that small increment, also into its distance from the water-section: the sum of these products, being divided by the solid contents of the volume displaced, will be the distance DE, of the water-section, from the centre of gravity of that volume.

The position of the vessel's centre of gravity G, depends

partly on the construction and equipment of the vessel, and partly upon the distribution of the lading and ballast, which circumstances therefore determine the distance GE, or the distance between the centre of gravity of the vessel, and that of the displaced volume.

These several conditions having been determined, the construction of the vessel's stability will be as in the former cases. Through the point E, draw the line EV parallel and equal to the line PQ; and, in EV, take ET to EV as the volume immersed by the inclination is to the entire volume displaced; or as A to V. Through the centre of gravity G, draw GU parallel to CH, and through the point T draw TZ perpendicular to GU. GZ is the measure of the vessel's stability, when inclined from the upright through the angle ASH.

The weight of the vessel and lading is found from the following proportion:* as 1 cubic foot is to V, the volume displaced, so is $\frac{1}{35}$ part of a ton to the vessel's weight, which will therefore be $= \frac{V}{35}$ ton.

The arithmetical operations required for ascertaining the sta-

* According to Mr. COTES, (Hydrostatics, page 73,) the specific gravity of sea water is $= 1.03$, when that of fresh water is $= 1$. And, since the weight of a cubic foot of rain water is 1000 oz. or $62\frac{1}{2}$ pounds avoirdupois, it will follow, that the weight of a cubic foot of sea water is $62.5 \times 1.03 = 64.375$ pounds avoirdupois. Mr. CHAPMAN, in his Treatise on the Method of finding the proper Area of the Sails for Ships of the Line, infers the weight of an English cubic foot of sea water to be 63.69 pounds avoirdupois. If an average between these results be taken, the weight of a cubic foot of sea water will be very nearly 64 pounds avoirdupois; and the weight of 35 cubic feet of sea water will be almost exactly one ton. According to the tables published by M. BRISSON, the specific gravity of sea water is 1.0263, when that of rain water is 1. By computing from this specific gravity, the weight of a cubic foot of sea water will be 64.14 pounds avoirdupois. 64 pounds avoirdupois is assumed as the average weight, in the ensuing computations.

bility of vessels, by the methods here described, are far from difficult, although they necessarily extend to some length; in order to give an illustration of these rules, by applying them to a particular vessel, I obtained, by favour of Messrs. RANDALL and BRENT, eminent constructors, a draught expressing the form and dimensions of a large ship,* built for the service of the East India Company. According to this draught, the vessel is divided into 33 vertical segments, by 34 sections, intersecting the longer axis at right angles, and at a common distance of 5 feet.†

The lengths of the ordinates entered in the annexed table‡ sufficiently define the form and magnitude of each of the 34 vertical sections; it will not therefore be necessary to represent their figures by separate drawings, since the constructions and calculations founded on them, for inferring results in any one section, are similar to those which are required in the other sections.

The greatest or principal section, which, according to this draught, intersects the longer axis at about 60 feet from the 1st section adjacent to the head, is represented by the figure BAO (fig. 31.): BA is the breadth at the water-line = 43.16 feet. BA is bisected in the point D; and DO, drawn through D perpendicular to BA, is the distance of the keel from the

* The ship CUFFNELLS.

† Mr. BRENT, jun. obligingly took the trouble, at my request, of delineating each of these sections on a large scale, and likewise of drawing and measuring the equidistant ordinates necessary for calculating the areas thereof, together with such additional lines as are required for constructing the measure of the vessel's stability, according to the principles delivered in the preceding pages.

‡ See Appendix.

water-section = 22.75 feet. The line DR = 22 feet, is divided into 11 equal parts, and, through the points of division, 12 ordinates* are drawn, parallel to the line BA, at the common distance of 2 feet.

The vessel is supposed to be inclined round the longer axis, at an angle of 30°, and the line NDW is drawn through the point D, inclined to BA, at an angle ADW = 30°: proceeding according to the solution which has been given, by measuring the line DW = 22.6 feet, DA = 21.58 feet, the area of the triangle ADW $= \frac{21.58 \times 22.6}{4} = 121.92$ square feet. Also, by mensuration, the line WA = 11.55: this line being divided into six equal parts, of 1.925 each, if ordinates are drawn at the points of division, perpendicular to the line WA, they are found to be as here stated.

	Ordinates. Pts. of a Foot.	Numbers.	Products.
a	= 0.00	1	0.00
b	= 0.15	4	0.60
c	= 0.30	2	0.60
d	= 0.43	4	1.72
e	= 0.38	2	0.76
f	= 0.23	4	0.92
g	= 0.00	1	0.00
		Sum	4.60

$\frac{1}{3}$ common interval = .642; and the area AWb = 4.6 × .642 = 2.95

By computing according to the Rule II. from the 7 ordinates given, of which the two extremes are = 0, the area of the curve space AWb = 2.95, which being added to the area ADW = 121.92, the area of the entire figure ADWb = 124.87. By similar calculations, the area of the figure BDNc is found to be = 133.68.

The areas of the figures ADWb and BDNc, being measured in each of the 34 vertical sections, are found to be as follows.

* The numerical measures of these lines are inserted in the table of ordinates; (see Appendix;) the numbers are entered in the 12th vertical section. It is not necessary to express them in the figure.

Vertical Sections	Areas of the Figures ADW*b* Square Feet.	Areas of the Figures BDN*c* Square Feet.
1	42.86	23.61
2	81.53	58.92
3	100.80	86.80
4	114.16	105.27
5	121.56	115.70
6	121.75	120.90
7	123.47	125.36
8	125.20	129.82
9	124.87	131.04
10	124.54	132.27
11	124.69	132.97
12	124.87	133.68
13	124.87	133.68
14	124.87	133.68
15	124.82	133.42
16	124.78	133.17
17	124.20	132.85
18	124.62	132.53
19	123.91	131.05
20	123.21	129.57
21	121.06	127.48
22	118.91	125.40
23	117.50	122.66
24	116.10	119.93
25	114.01	116.88
26	111.91	113.83
27	108.96	109.81
28	106.01	105.80
29	101.82	98.92
30	97.24	91.71
31	92.41	79.95
32	86.31	66.06
33	81.60	48.20
34	68.35	17.92
	3767.77	3700.84

The vertical sections intersect the longer axis at a common interval of 5 feet. By computing, according to the Rule III. from the areas of the 34 figures ADW*b* here given, together with the common interval of 5 feet, a result will be obtained, which approximates very nearly to the solid contents of the volume between the planes DA, DW, and the intercepted side of the vessel, throughout the entire length of it.

Making, therefore, according to Rule III. the sum of the first and last area - - $= S =$ 111.21

And the sum of the 4th, 7th, 10th, 13th, &c. area (except the 34th,) $= P =$ 1167.58

$S + P =$ 1278.79

Sum of all the areas - $=$ 3767.77

Sum of the 2d, 3d, 5th, 6th, &c. areas - $= Q =$ 2488.98

Solid contents of the volume between the planes DA, DW, and the intercepted side of the vessel $= \overline{S + 2P + 3Q} \times \frac{15}{8} =$ 18587.5 cubic feet.

From the 34 areas of the figures BDN*c*, as entered in the table, by computing according to the Rule III. the solid contents are obtained, of the volume between the planes DB, DN, and

the intercepted side of the vessel: for, observing the notation already described, and applied to the areas BDN*c*.

$$S = 41.53$$
$$P = 1188.83$$
$$Q = 2470.48$$

The solid contents of the volume between the planes DB, DN, and the intercepted side of the vessel,

is $= \overline{S + 2P + 3Q} \times \frac{15}{8} = 18432$

Volume between the planes DA, DW, and the intercepted side of the vessel $= 18587$

Difference - $=$ 155 cubic feet.

The area of the section passing through all the lines CDW, is found from having these lines given by mensuration, and the common interval of 5 feet between them. This area is $= 7106$ square feet. The distance DY * between the plane NW and the plane CH, which coincides with the water-section, when the vessel is inclined 30° from the upright, is $= \frac{155}{7106} = .022$ parts of a foot, and the distance DS † $= \frac{.022}{\text{sin. } 30^\circ} = .044$ parts of a foot, or little more than $\frac{1}{2}$ an inch: a quantity of which it is unnecessary to take any account in this construction. ‡

When, therefore, the vessel is inclined from the perpendicular, through an angle of 30° round the longer axis, the water's surface will pass through the middle point D of the line BA, in all

* Page 283. † Ibid.

‡ The form of the sides above and beneath the water line, in this vessel, causes the points D and S almost to coincide; but, in vessels differently formed the distance of these points is often considerable. The present instance points out the method of constructing the line DS, as distinctly as if it was of greater magnitude.

the sections; the volume immersed, by the inclination on the side ADW, being equal, in a practical sense, to the volume which emerges on the side BDN. Let each or either of these volumes be denoted by the letter A = 18509 cubic feet, being the average value between 18432 and 18587. Through the points D, in all the sections, draw lines Ff perpendicular to the lines NW; the same plane passes through all the lines Ff: in the next place, the distance between the centres of gravity of the volumes immersed, and caused to emerge, in consequence of the vessel's inclination, estimated in the direction of a line NW, perpendicular to the plane Ff, is to be obtained. To effect this, the line Dl*, in the principal section, is found by mensuration to be $= 13.8$, $lU = 7.03$; and the area † $AWb = 2.95$. The area $ADWb$ has been found $= 124.87$. ‡ Wherefore, according to the preceding solution, the distance $lL = \frac{2.95 \times 7.03}{124.87} = 0.17$, which being added to the line $Dl = 13.8$, the sum will be $DL = 13.97$, and the product arising from multiplying this line into the area $ADWb$ will be $13.97 \times 124.87 = 1742$. Similar products being obtained, arising from multiplying the several areas $ADWb$ into the perpendicular distances of their centres of gravity from the plane Ff, also the several products arising from multiplying the areas $BDNc$ into the perpendicular distances of their centres of gravity from the plane Ff, in each of the 34 vertical sections, the results will be as expressed in the adjacent table.

* The point M is the centre of gravity of the triangle ADW, as in the general construction and solution: Ml is drawn through M, perpendicular to NW: the line lL is found according to the method described in the same general solution.

† Page 288. ‡ Ibid.

Vertical Sections	Products on the Side ADH *b*	Products on the Side BDC *c*
1	359	143
2	929	562
3	1269	1010
4	1522	1342
5	1683	1542
6	1699	1669
7	1723	1766
8	1747	1853
9	1739	1873
10	1731	1892
11	1736	1912
12	1742	1931
13	1742	1931
14	1742	1931
15	1727	1930
16	1713	1929
17	1724	1915
18	1736	1901
19	1719	1866
20	1702	1832
21	1660	1798
22	1618	1764
23	1591	1705
24	1564	1646
25	1516	1584
26	1468	1522
27	1415	1434
28	1363	1346
29	1290	1220
30	1199	1088
31	1114	881
32	1010	673
33	919	415
34	708	102
	50119	49908

Suppose the volume immersed, by the inclination on the side ADW *b*, to be divided into very thin laminæ, or solids, the bases of which are the areas of the successive figures ADH *b*, and the thickness a small increment of the longer axis; by applying the Rule III. to the products in the adjacent table, corresponding to the figures ADW *b*, we shall obtain the sum of all the products, arising from multiplying each of these thin solids into the perpendicular distance of its centre of gravity from the plane $Ff = \overline{S+2P+3Q} \times \frac{15}{8} = 254367$; which sum of products being divided by the solid contents of the said volume, or 18509, will be the distance DQ, of the centre of gravity of the volume ADW *b* from the plane $Ff = \frac{254367}{18509} = 13.78$; and, by a similar calculation, the distance DP from the plane F *f*, of the centre of gravity of the volume on the side BDN *c*, caused to emerge by the inclination, will be $\frac{250627}{18509} = 13.54 = DP$.

The sum of these two lines, DQ + DP, or PQ, will be = 27.32 feet, which is the distance of the centres of gravity of the volumes immersed and emerged, in consequence of the vessel's inclination, esti-

mated in the direction of the line NW, perpendicular to the plane Ff: let the line PQ be denoted by the letter b.

The solid contents of the entire volume displaced are next to be measured, from having first obtained the areas of the 12 horizontal sections, intersecting the vertical axis at the common interval of 2 feet, and dividing the immersed volume into 11 horizontal segments. The areas of the several horizontal sections are measured by Rule III. from having given the ordinates drawn in the said sections, parallel to the water's surface from head to stern, at the common interval of 5 feet.

The mensuration of the area of the horizontal section 12, which coincides with the water's surface, from the ordinates entered in the annexed * table, is as follows:

Vertical Sections	Ordinates of the Horizontal Section 12. Feet.	Vertical Sections	Ordinates of the Horizontal Section 12. Feet.
1	10.78	19	21.48
2	16.00	20	21.32
3	18.40	21	21.22
4	19.84	22	21.05
5	20.54	23	20.82
6	20.94	24	20.61
7	21.20	25	20.44
8	21 38	26	20.15
9	21.48	27	19.85
10	21.50	28	19.58
11	21.56	29	19.25
12	21.58	30	18.77
13	21.56	31	18.21
14	21.56	32	17.52
15	21.56	33	16.50
16	21.55	34	12.95
17	21.53		
18	21.51		674.19

According to the Rule III.

$$S = 23.73$$
$$P = 206.41$$
$$Q = 444.05$$

the area of half the horizontal section $12 = \overline{S + 2P + 3Q} \times \frac{15}{8} = 3316.3$, and the total area of the horizontal section $12 = 6633.6$ square feet.

The areas of the 12 horizontal sections, computed by Rule III. from the ordinates, as expressed in the table inserted in the Appendix, are as follows.

* See Table of Ordinates, in the Appendix.

Water-Lines.	Areas of the Horizontal Sections. Square Feet.
12	6633.6
11	6568.4
10	6499.3
9	6324.0
8	6238.0
7	5948.8
6	5687.8
5	5353.4
4	4906.6
3	4298.6
2	3417.0
1	925.0
	62800.5

The solid contents of the volume between the sections 12 and 2, is measured by the Rule II. by making the sum of the 12th and 2d areas $= S$ the sum of the 11th, 9th, 7th, 5th, and 3d, $= P$ the sum of all the other areas - $= Q$ the common interval between the areas being two feet.

The solid contents of the volume between the sections 12 and 2, is $\overline{S + 4P + 2Q} \times \frac{2}{3}$ $= 113791.2$ cubic feet.

The contents of the volume between the section 2 and 1, may be obtained by the Rules II. and III. For, by the Rule III. the contents between the sections 4 and 1, is found to be $= 21733.8$

By Rule II. the contents between the sections 4 and 2 is - - - - $= 17012.0$

Contents between the sections 2 and 1 $= 4721.8$

Contents between the sections 12 and 2 $= 113791.2$

Contents between the sections 12 and 1 $= 118513.0$

To the volume thus determined, the contents of the space between the horizontal section 1 and the keel, are to be added. To measure this volume, the areas must be first obtained of the 34 vertical sections which are included between the first ordinate in each vertical section and the keel; which areas are found, by the methods of mensuration already described, to be as in the annexed table.

Vertical Sections	Areas between the first Ordinate and the Keel. Square Feet.
1	
2	
3	
4	2.4
5	2.8
6	4.6
7	5.6
8	6.6
9	7.4
10	9.3
11	10.5
12	10.5
13	10.5
14	10.5
15	10.5
16	10.5
17	9.8
18	9.2
19	8.5
20	8.0
21	6.0
22	5.3
23	4.8
24	4.3
25	3.7
26	3.0
27	2.4
28	1.9
29	1.6
30	1.4
31	1.2
32	1.1
33	1.0
34	1.0
	175.9

Applying the Rule III. to these areas, and making the common distance between them $= r = 5$ feet, the solid contents between the first horizontal section and the keel is found to be $\overline{S + 2P + 3Q} \times \frac{3r}{8} = 871.0$ feet.

	Feet.
Contents between the horizontal sections 12 and 1 - -	118513.0
Contents between the first section at the keel* - - -	871.0
Total contents of the volume immersed between the first and thirty-fourth vertical section - - -	119384.0

Let this volume be represented by the letter V.

By the preceding computations, the line PQ $= b$ was found $= 27.32$, and the volume immersed by the vessel's inclination, or $A =$ 118509. From these determinations, the line ET is inferred; for, according to the general theorem,

as V : A :: b : ET or

119384 18509 27.32 4.23.

wherefore ET $= 4.23$.

* The rules here employed for measuring the volume displaced, cannot be applied to the irregular parts of the vessel, adjacent to the head and stern, without constructing and measuring the ordinates by which their forms are defined: the same observation applies to the mensuration of the body of the keel and of the rudder. But, as these additional volumes bear a small proportion to the entire volume displaced, they may be determined by an estimate founded on the draught of the vessel, sufficiently near the truth, without taking the trouble of a more rigorous calculation by equidistant ordinates.

To infer the measure of stability from this value of the line ET, it will be necessary to have given the distance GE, between the centre of gravity of the vessel, and the centre of gravity of the volume of water displaced. The position of this latter centre is regulated entirely by the form and dimensions of the body under water; and, on this account, is to be considered as a point absolutely fixed, in respect of the water-section, or other given plane. But the position of the vessel's centre of gravity being regulated, partly by the construction and equipment of the vessel, and partly by the distribution of the lading and ballast, can be assumed on the ground of supposition only; unless in cases where the position of this point has been actually ascertained. In some vessels, the distance GE has been measured, and found equal to about $\frac{1}{8}$ part of the greatest breadth at the water-line: without knowing what the real distance of the two centres of gravity G and E may be, in the ship of which the dimensions are here given as subjects of calculation, the distance GE may be estimated (merely for the purpose of exemplifying the preceding rules) at $\frac{1}{8}$ of the breadth BA, or $\frac{43.16}{8} = 5.39$. Consequently, the inclination of the vessel from the upright being $30°$, $GE \times \sin. 30° = \frac{5.39}{2} = 2.69 = ER$: which being subtracted from $ET = 4.23$, will leave TR or $GZ = 1.53$ feet, the measure of the vessel's stability, when inclined round the longer axis through an angle of $30°$.

The solid contents of the volume displaced being 119384 cubic feet, the weight of the vessel and contents will be equal to that of 119384 cubic feet of sea water; which, allowing 35 cubic feet to each ton, will amount to 3410 tons. According to this determination, the force of stability to turn the vessel round the longer axis, when inclined from the upright through an angle of $30°$, is a force of 3410 tons, acting at a distance of 1.53 feet

from the axis; which force is equivalent to a weight or pressure* of 241 tons, acting at a distance of 21.58, or half the breadth at the water-line from the axis.

In this computation, the distance GE, between the centres of gravity G and E, has been assumed without considering the absolute position of these points, in respect to the water-section or keel. But the distance DE, or OE, ought to be known, since the point E being fixed in the same vessel, when the weight is given, and the centre of gravity G being within certain limits moveable, the adjustment of this centre, by means of the lading and ballast, will be better regulated, if the position of the point E be first ascertained: the distance DE will be found from having given the areas of the 12 horizontal sections, and the contents of the volume between the section 1 and the keel.

Suppose the areas of 12 horizontal sections of the vessel to be given, as they are expressed in page 294; let the displaced volume be conceived divided into laminæ, or very thin solids, of which the bases are the areas of the successive horizontal sections, and the thickness a small increment of the vertical axis. The sum of the products arising from multiplying each of these segments into its perpendicular distance from the section 12, also a similar sum of products for the solid contents adjacent to the keel, will be found from the computation subjoined.

* This equivalent weight is not here supposed to be counterbalanced by the wind, which probably never acts with sufficient force to keep a vessel of this weight inclined permanently from the upright to so great an angle. But the measure of force which acts to turn the vessel round the longer axis, when inclined to this angle of 30°, is precisely that which is here stated, according to the given conditions.

Water-lines, or horizontal Sections.	Areas of the horizontal Sections. Square feet.	Distances from the Section 12.	Products of each area, multiplied into its distance from the Section 12.	Numbers for computing according to the Rule II.	Products.
12	6633.6	0	00000.0	1	
11	6568.4	2	13136.8	4	52547.2
10	6499.3	4	25997.2	2	51994.4
9	6324.0	6	37944.0	4	151776.0
8	6238.0	8	49904.0	2	99808.
7	5948.8	10	59488.0	4	237952.
6	5687.8	12	68253.6	2	136507.2
5	5353.4	14	74947.6	4	299790.4
4	4906.6	16	78505.6	1	78505.6
				Sum of the products =	1108880.8

The common interval between the sections being 2 feet, the sum of all the products arising from multiplying each thin horizontal segment contained between the section 12 and 4, into its distance from the water-section, will be $\frac{2}{3}$ of 1108880.8 = 739253, by the Rule II.

				Numbers for computing according to Rule III.	
4	4906.6	16	78505.6	1	78505.6
3	4298.6	18	77374.8	3	232124.4
2	3417.0	20	68340.0	3	205020.0
1	925.0	22	20350.0	1	20350.0
					536000.0

Sum of the products arising from multiplying each thin horizontal segment between the section 4 and 1, into its distance

from the section 12, $= \frac{3}{4} \times 536000$, by the Rule III. $=$ 402000
Sum of the products between section 12 and 4 $=$ 739253
*The contents of the volume between the keel and the ordinate 1, multiplied into the distance of its centre of gravity from the section 12 - $=$ 19465

Sum of the products arising from multiplying each horizontal evanescent solid into its distance from the water-section - - - $=$ 1160718

The sum of products, thus found, divided by the entire volume displaced, will be the distance of the centre of gravity of that volume from the water-section, or

$$DE = \frac{1160718}{119384} = 9.7224 \text{ feet.}$$

If the distance between the centres of gravity G and E should be assumed $\frac{1}{8}$ of the breadth BA at the water-line, or 5.39 feet, this distance being subtracted from 9.22 feet, will be the distance of the vessel's centre of gravity beneath the water-section, or DG $= 3.83$ feet.

To determine the limiting point or metacentre W, above which if the centre of gravity G should be raised the vessel will overset, it is only necessary to compute, by means of the Rule II. or III. the value of the line $EW = \frac{\text{Fluent of } BA^3 \times \dot{z}}{12V}$: where z represents any portion of the longer axis: AB $=$ the breadth of the water-section, at the distance z from the initial point where the mensuration commences: $\dot{z} =$ a small increment of z : V $=$ the solid contents of the volume displaced.

By computing the cubes of each ordinate of the water-section, drawn at the common interval of 5 feet, as represented in the

* Equal to the sum of the products arising from multiplying each thin horizontal segment between the section 1 and the keel, into its distance from the water-section.

table* of ordinates, the total sum of these cubes is = 276573.21
Cube of the 1st ordinate, or 10.78 = 1252.73
Cube of the 34th ordinate, or 12.95 = 2171.75

Sum S = 3424.48

Sum of the cubes of the 4th, 7th, 10th, &c. 28th, and 31st = P = 88628.41

S + P = 92052.89

Sum of the cubes of the 2d, 3d, 5th, 6th, &c. 32d, 33d = Q - - - - = 184520.32

$S + 2P + 3Q = 734242.26$, this sum $\times 15$, will be = fluent of $BA^3 \times \dot{z} = 11{,}013{,}633.9$; and since $V = 119384.0$, $EW = \frac{11{,}013{,}633.9}{12 \times 119384.} = 7.688$ feet.

In this vessel, the centre of the immersed volume E is 9.72 feet beneath the water's surface; it follows, that the metacentre will be 2.03 feet beneath the water's surface.

The total weight of a vessel and contents is inferred from knowing the volume of water displaced by the vessel, the solid contents of which space have been calculated in the preceding pages, from the areas of the 12 horizontal sections intersecting the vertical axis at a common interval of 2 feet. By similar calculations, we may determine the several weights of tonnage which will cause the vessel to sink to any different depths, estimated by the horizontal line or section which is coincident with the water's surface.

The solid contents of the volume included between the horizontal sections 12 and 9, is found, by the Rule III. to be 39120 cubic feet; displacing a weight of water (allowing 35 feet to

* See Appendix.

a ton) of 1117.6 tons: the volume between the sections 11 and 9, is found, by the Rule 11. to be = 25926, displacing 741 tons of water: the volume between the sections 12 and 11 will therefore displace a weight = 377 tons.

By similar computations, the following results are obtained.

From the water-section.	Difference of tonnage.	From	Difference of tonnage.
12 to 11	377 tons.	12 to 11	377 tons.
11 to 10	374	12 to 10	751
10 to 9	367	12 to 9	1118
9 to 8	357	12 to 8	1475
8 to 7	348	12 to 7	1823
7 to 6	333	12 to 6	2156

If any one of the adjustments determining the stability of a vessel should be altered, the several other conditions on which that power depends, will most commonly experience corresponding changes, the effects of which it is not easy to estimate, without some reference to the theory of stability. If the weight of the Cuffnells and contents should be diminished 751 tons; or rather if another vessel, constructed in all respects like the Cuffnells, should be loaded by 751 tons less weight, the following changes will take place, by which the stability is principally affected; one of which is additive to, and the others subtractive from, the stability of the vessel.

1st. The section of the water will be nearer to the keel by 4 feet; so to coincide with the horizontal section 10 instead of 12.

The centre of gravity of the displaced volume will be nearer to the keel than before by - - 2.19 feet.

Admitting, therefore, in the first instance, the

centre of gravity* of the vessel and contents to remain at the same distance from the keel, the distance of that centre from the centre of the displaced volume will be increased by 2.19 feet
and this change will operate to diminish the stability.
2dly. The total weight being less, will have the effect of diminishing the stability, in the proportion of - - - 3410 to 2660
3dly. The breadths of the vessel at the water-line are less than before, being the double ordinates in the horizontal † section 10, instead of those in the horizontal section 12. By this change, the stability will also be diminished.
4thly. The volume displaced being less, in consequence of the diminished tonnage, in the proportion of 2660 to 3410, the stability will be augmented by this change; not in the proportion of 2660 to 3410, but in a proportion considerably greater; yet not sufficient to counterbalance the effect of the alterations by which the stability is diminished: on the

* Let the distance of the centre of gravity of any body, or system of bodies, from a given plane, be denoted by the letter D: if weights are taken away from different parts of the system, in such a proportion that the sum of the products arising from multiplying each removed weight into its distance from the given plane, divided by the sum of the removed weights, may be equal to the distance D, the distance of the centre of gravity of the remaining weights, from the same plane, will also be the distance D: that is, the distance of the common centre of the whole system, from the given plane, will not be affected by the removal of any portion of the weights, according to the conditions here described.

† See Appendix.

whole, these alterations will diminish the stability of the vessel, when inclined to a given small angle, in the proportion of about* - 100 to 67

In this estimate, the vessel's centre of gravity has been supposed to remain at the same distance from the keel at which it was situated in the Cuffnells, and consequently more remote from the centre of the displaced volume by 2.19 feet: suppose the vessel's centre of gravity to be depressed 2.19 feet, by altering the distribution of the lading and ballast, so as to be at the same distance from the centre of the displaced volume, as in the Cuffnells: the effect of this alteration will be entirely additive to the stability, which will be increased in the proportion of 47 to 100.

An increase in the proportion of 47 to 100, combined with a diminution in the proportion of 100 to 67, is, on the whole, an increase of stability, in the proportion of 47 to 67, or about† 7 to 10

It might be difficult to depress the vessel's centre of gravity

* The subject of these observations being the relative stabilities of the two vessels, they are supposed to be inclined from the upright to the same angle, which may be assumed of any magnitude, either great or small: the latter supposition is here adopted, which is well suited to the purpose of general illustration. But nothing can be inferred from these results, respecting the stabilities, when the angles of inclination are considerable; which are to be obtained from computations founded on the methods which have been described in the preceding pages.

† This proportion might have been immediately inferred from one computation only; but, by calculating the effects of diminishing the vessel's weight separately, the increase and diminution of stability, arising from the alteration of the several conditions, are more distinctly expressed.

through so great a space as 2.19 feet; and the increase of stability which would ensue from it, may perhaps not be necessary. If it should be required that the stability of the vessel, when the weight is diminished 751 tons, shall be just equal to that of the Cuffnells, this will be effected by adjusting the centre of gravity lower than its original position, by only .97 parts of a foot.

These determinations relate to the vessel's stability in respect to the longer axis. But the position of the shorter axis, round which the ship revolves in pitching, and of the vertical axis, round which it is caused to turn by any horizontal force not passing through the vertical axis, will also experience some change in consequence of diminishing the vessel's weight. For the centre of gravity of the volume displaced, being necessarily in the same vertical line with the vessel's centre of gravity when it floats quiescent, fixes the position of the latter point, in respect to the ship's length, when floating on an even keel. And since the alteration of the water-section, of 4 feet in height, causes the centre of gravity of the displaced volume to approach nearer to the head of the vessel by about $\frac{1}{2}$ a foot, both the shorter horizontal axis and vertical axis of the vessel must experience the same change of position: the former alteration affects the motion of the vessel in pitching, and the latter somewhat increases the action of the rudder in turning the ship, and also affects the motion of the vessel, in turning to and from the wind, by causes independent of the rudder.*

These observations point out the alterations of stability, in

* By altering the distance of the centre of gravity from the points of application, in the longer axis, at which the water's resistance and force of the wind, when not exactly balanced, act on the vessel.

consequence of diminishing the tonnage of the vessel, without entering into any consideration how far such changes are, on the whole, beneficial or otherwise.

It is here necessary to observe, that the force of stability and the measure of it, the subject of investigation in the preceding pages, is wholly independent of the water's resistance, which co-operates with the vessel's stability only while it is inclining, and wholly ceases as soon as the vessel has attained to the greatest inclination, at which it is supposed permanently to remain in a state of equilibrium; the inclining force being exactly balanced by the force of stability. This observation will obviate any difficulty that might possibly occur from the principle stated in page 213; which is, that if the shape of the zone WHFC, (fig. 1 and 2.) comprehending that portion of the sides of a vessel which may be immersed under, and may emerge above, the water's surface, should be the same in two vessels, the stability will be the same at all equal angles from the upright, whatever shape be given to the form of the volume immersed, which is situated beneath the said zone, provided the vessels be in other respects similarly constructed and adjusted: if, for instance, the keel of one vessel should be very deep under the body of the vessel, the keel of the other being of the ordinary dimensions, the deeper keel will oppose an increased resistance to the inclination of the vessel only while it is inclining, so as make it heel slower; but will not alter the angle of permanent inclination caused by a given force of the wind, or other uniform power; which inclination depends entirely on the stability, which has been determined in the preceding pages, and has no relation to the resist-

ance of the water, which arises from the vessel's motion round its longer axis.

The object of the preceding propositions, and inferences founded on them, has been rather to establish general principles, which may be of use in forming plans of construction, than to investigate what modes of construction are the most advantageous; a discussion more extensive than would be consistent with the subject here proposed to be considered, which relates to the stability of vessels only.

The practice of navigation requires the co-operation of many qualities in vessels, the laws and powers of which, considered as acting either separately or conjointly, it is the employment of theory to investigate. In respect to the construction of ships, it is obvious that no one of the component qualities can be regulated, without paying attention to all the others; because, by increasing or diminishing any of the powers of action, the others are commonly more or less influenced. It has been shewn, by the propositions demonstrated in these pages, that there are many practical methods by which the stability of vessels, at any given angle from the upright, may be augmented; a circumstance which gives to the constructor great choice of means for regulating this power, according to the particular service for which the ship is designed; for it is not every mode that will be advantageous. The several varieties of form and adjustment by which stability is increased, may be so unskilfully combined, that, in consequence of the very means used to obtain that essential quality, either the ship shall not steer well, or shall drift too much to leeward, or shall be liable to sudden and irregular motions in rolling, by which

the masts are endangered; or those angular oscillations of the ship shall be performed round an axis situated so much beneath the water's surface, that the motion of rolling shall be excessive and laborious. It is the proper use of theory, or right principle, whencesoever derived, so to adapt the means to the end proposed, that the required stability shall be imparted, without producing inconveniences of any kind, or such only as are unavoidable, and are the least prejudicial: the same observation applies to the other qualities of vessels. By duly combining the whole, ships are constructed so as to fulfil the purposes of navigation.

APPENDIX.

Note to the Investigation, Pages 232, 233.

$$\sqrt{\text{sine } O \times \text{sine } P} = \frac{\sqrt{1 - \text{tang.}^2 \frac{1}{2} F \times \text{tang.}^2 S}}{\text{sec.} \frac{1}{2} F \times \text{sec.} S}$$

This is investigated in the following manner:

Let R be put to denote a right angle, the other notation being the same as in page 232;

Then the angle $DWP = \frac{1}{2}F + P$; also $DWP = R - S$ wherefore $\frac{1}{2}F + P = R - S$ and $P = R - \frac{1}{2}F - S$, and, since $O = 2R - F - P$, it follows that $O = R - \frac{1}{2}F + S$; consequently

$$\text{sin. } P = \text{cos. } \overline{\tfrac{1}{2}F + S}$$

$$\text{sin. } O = \text{cos. } \overline{\tfrac{1}{2}F - S}$$

and

$$\text{sin. } O \times \text{sin. } P = \text{cos. } \overline{\tfrac{1}{2}F + S} \times \text{cos. } \overline{\tfrac{1}{2}F - S}$$

$$= \text{cos.}^2 \tfrac{1}{2}F \times \text{cos.}^2 S - \text{sin.}^2 \tfrac{1}{2}F \times \text{sin.}^2 S$$

or because $\text{cos.}^2 S = 1 - \text{sin.}^2 S$

$$\text{sin. } O \times \text{sin. } P = \text{cos.}^2 \tfrac{1}{2}F \times \overline{1 - \text{sin.}^2 S} - \text{sin.}^2 \tfrac{1}{2}F \times \text{sin.}^2 S$$

or $\text{sin. } O \times \text{sin. } P = \text{cos.}^2 \frac{1}{2}F - \text{sin.}^2 S$

$$= \frac{\text{sec.}^2 \frac{1}{2}F \times \text{sec.}^2 S}{\text{sec.}^2 \frac{1}{2}F \times \text{sec.}^2 S} \times \overline{\text{cos.}^2 \tfrac{1}{2}F - \text{sin.}^2 S}$$

or

$$\text{sin. } O \times \text{sin. } P = \frac{\text{sec.}^2 S - \text{sec.}^2 \frac{1}{2}F \times \text{tang.}^2 S}{\text{sec.}^2 \frac{1}{2}F \times \text{sec.}^2 S}$$

$$= \frac{1 + \text{tang.}^2 S - \overline{1 + \text{tang.}^2 \frac{1}{2}F} \times \text{tang.}^2 S}{\text{sec.}^2 \frac{1}{2}F \times \text{sec.}^2 S}$$

or

$$\text{sin. } O \times \text{sin. } P = \frac{1 - \text{tang.}^2 \frac{1}{2}F \times \text{tang.}^2 S}{\text{sec.}^2 \frac{1}{2}F \times \text{sec.}^2 S}$$

Finally,

$$\sqrt{\text{sin. } O \times \text{sin. } P} = \frac{\sqrt{1 - \text{tang.}^2 \frac{1}{2}F \times \text{tang.}^2 S}}{\text{sec.} \frac{1}{2}F \times \text{sec.} S}.$$

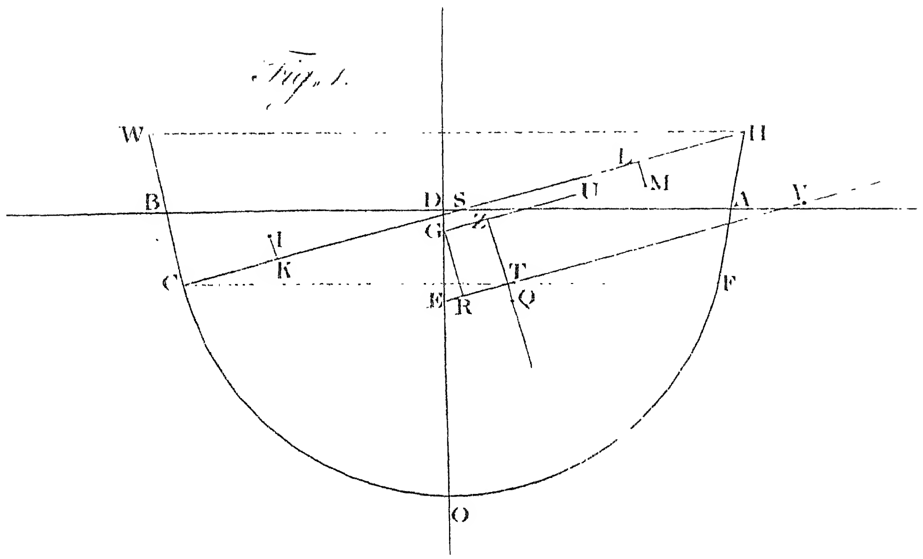
Fig. 1.
W H B D S L M U A Y I G Z K C T E R Q F O

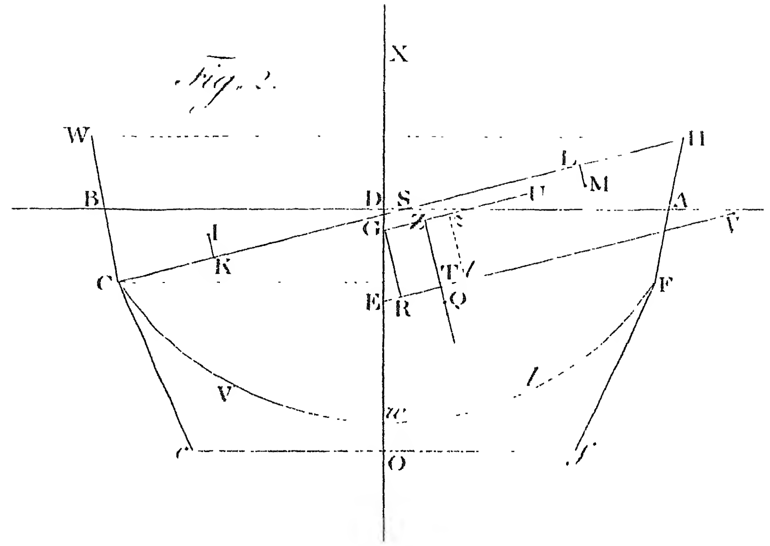
Fig. 2.
X W H B D S L M U A I G Z V K C T E R Q F V w O

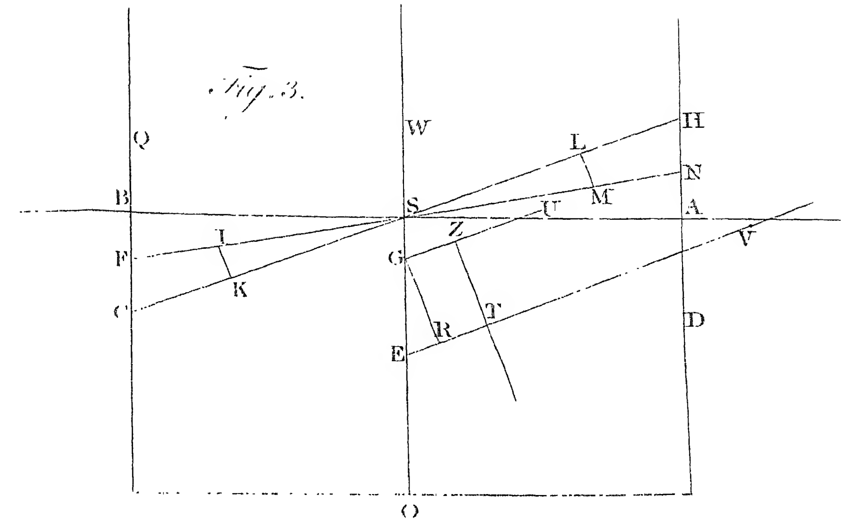
Fig. 3.
Q W H L N B S M U A F L G Z V C K R T D E O

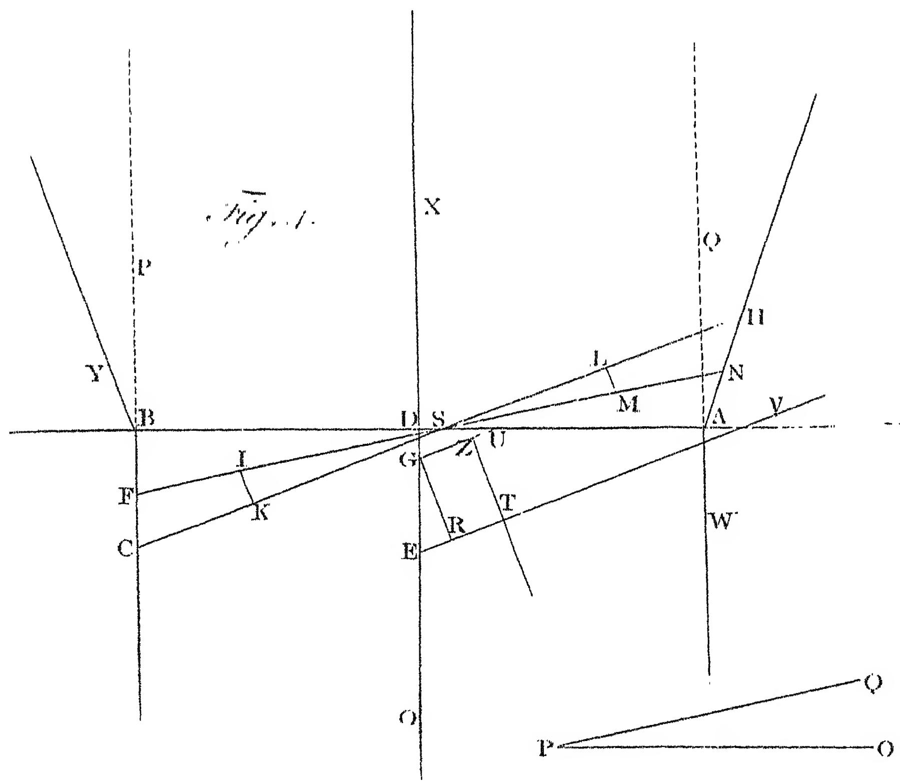
Fig. 4.
X P O H Y L N B D S M A V I G Z U F K T C R E W O Q P O

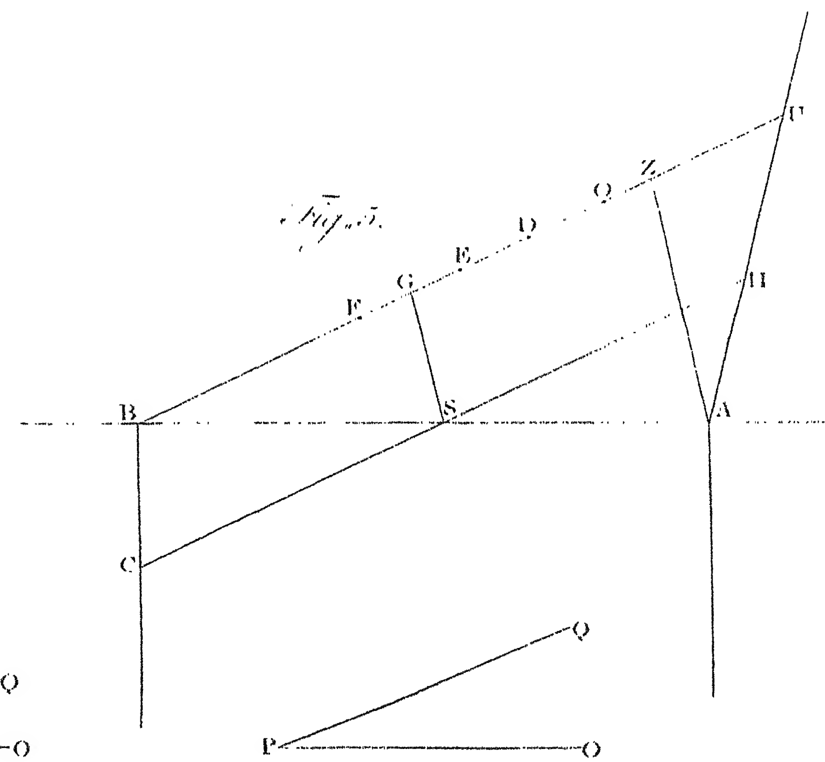
Fig. 5.
U Z Q D E G F H B S A C Q P O

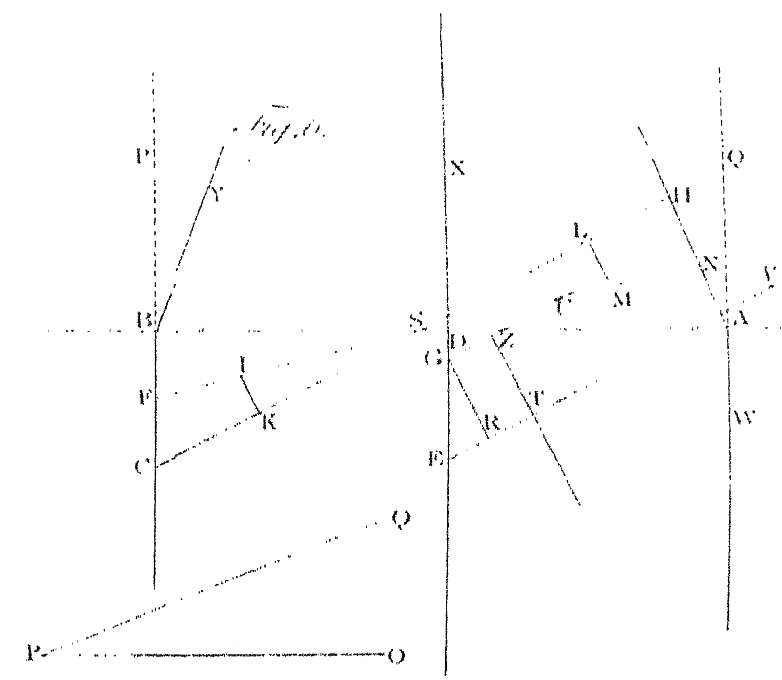
Fig. 6.
P Y X Q H L N U M B S D A Z I G F T R K W C E Q P O

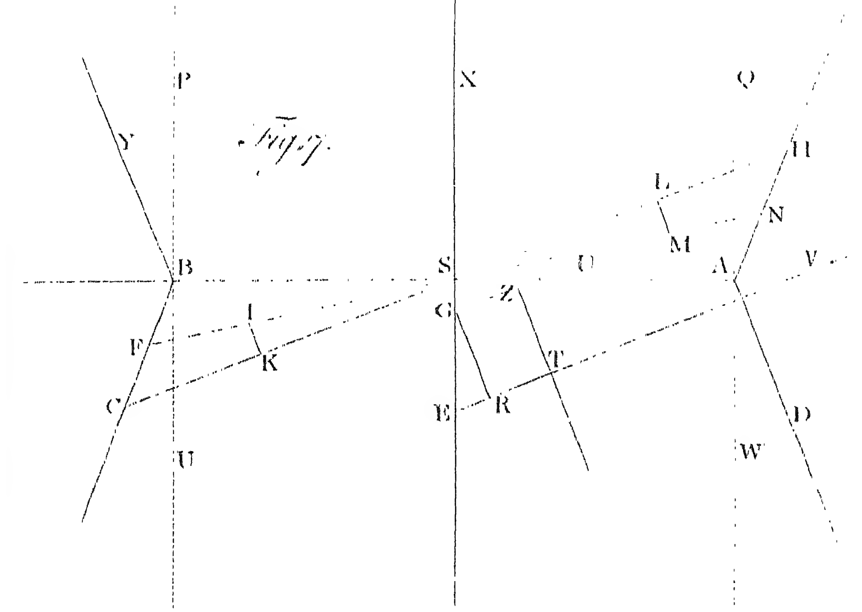

Fig. 7.

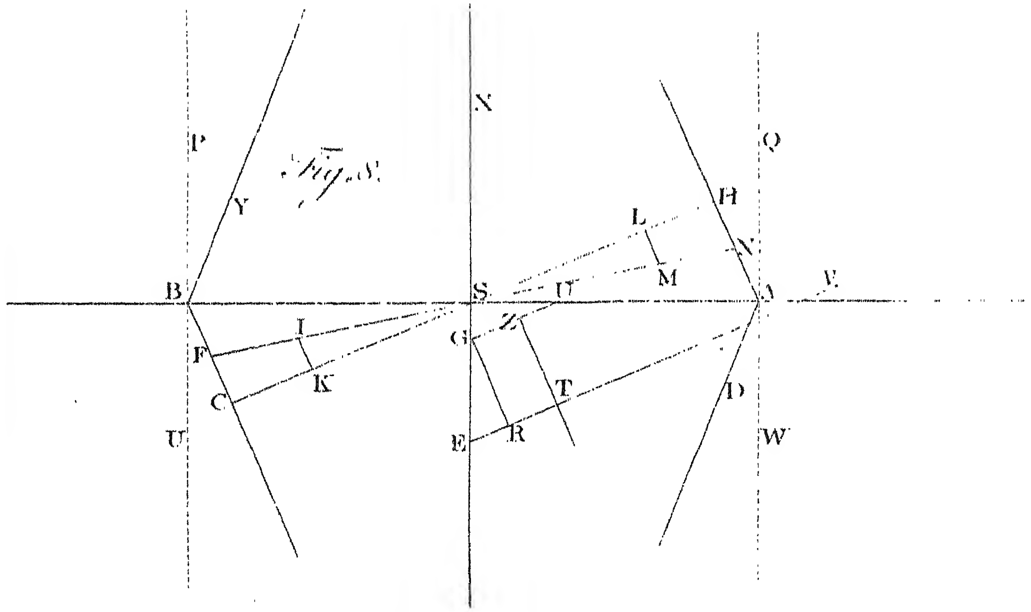

Fig. 8.

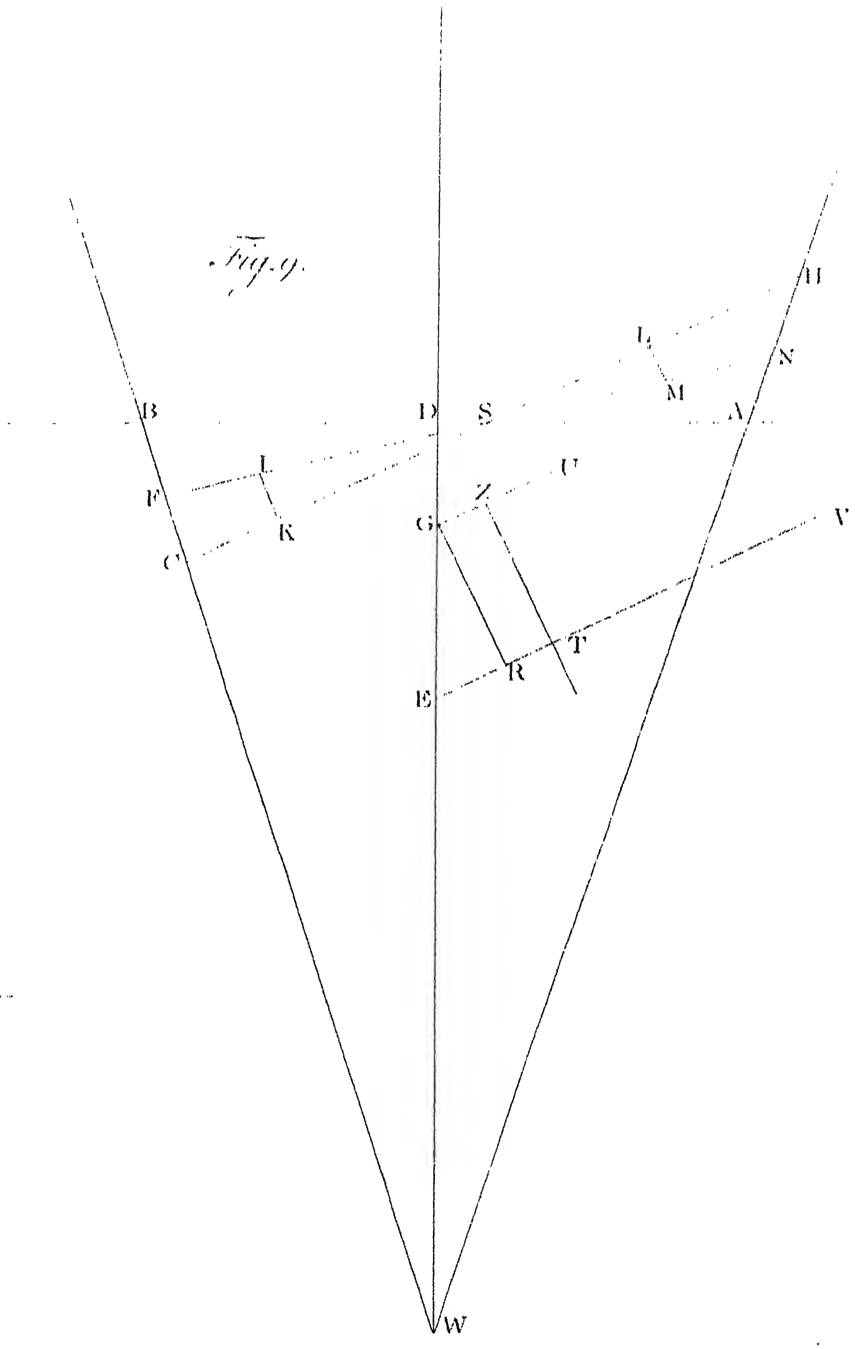

Fig. 9.

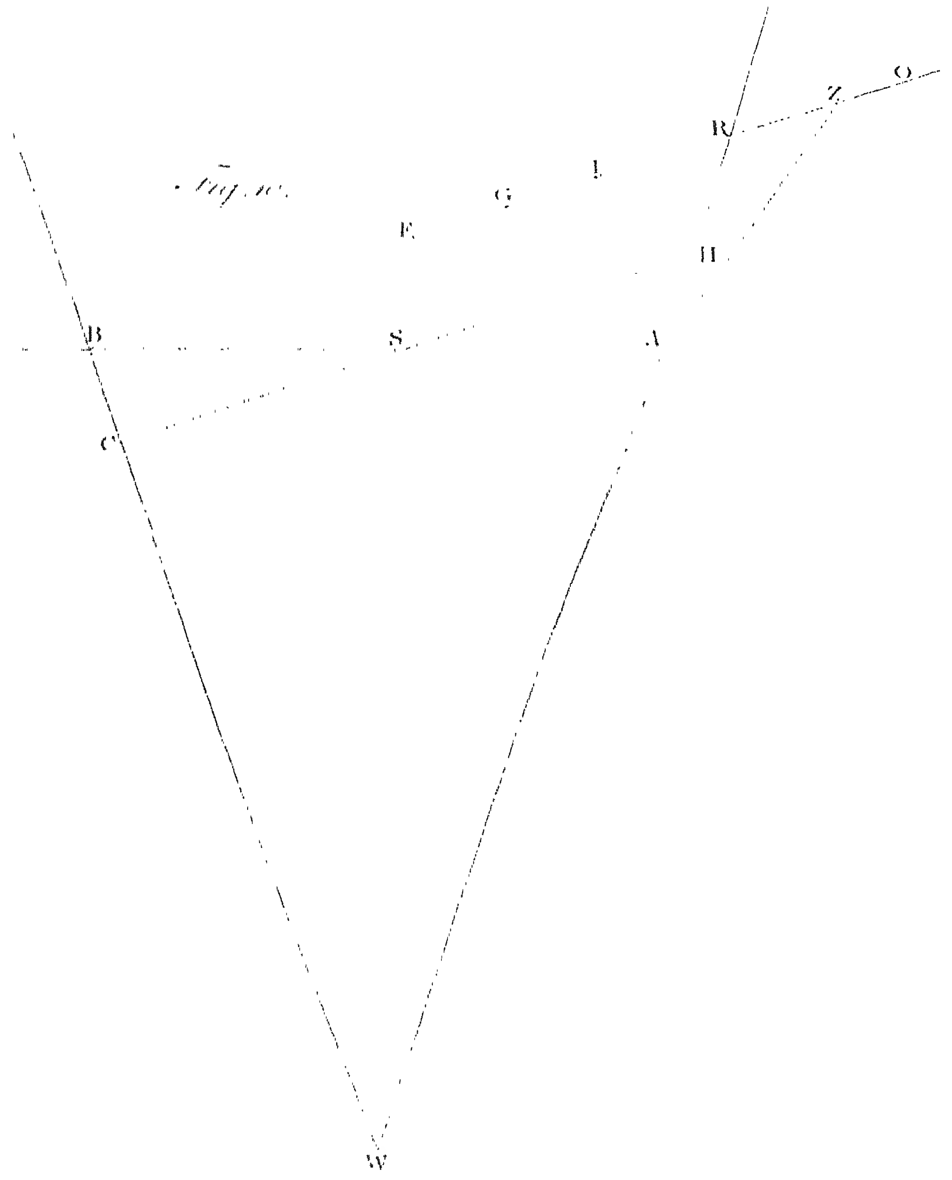

Fig. 10.

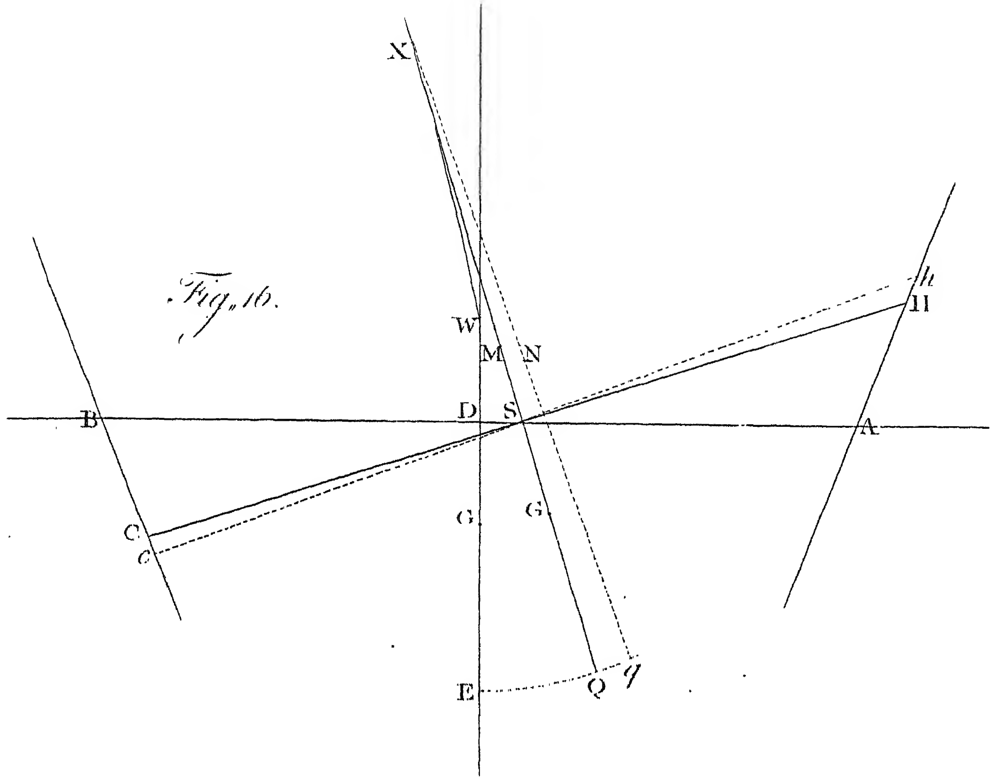

Fig. 16.

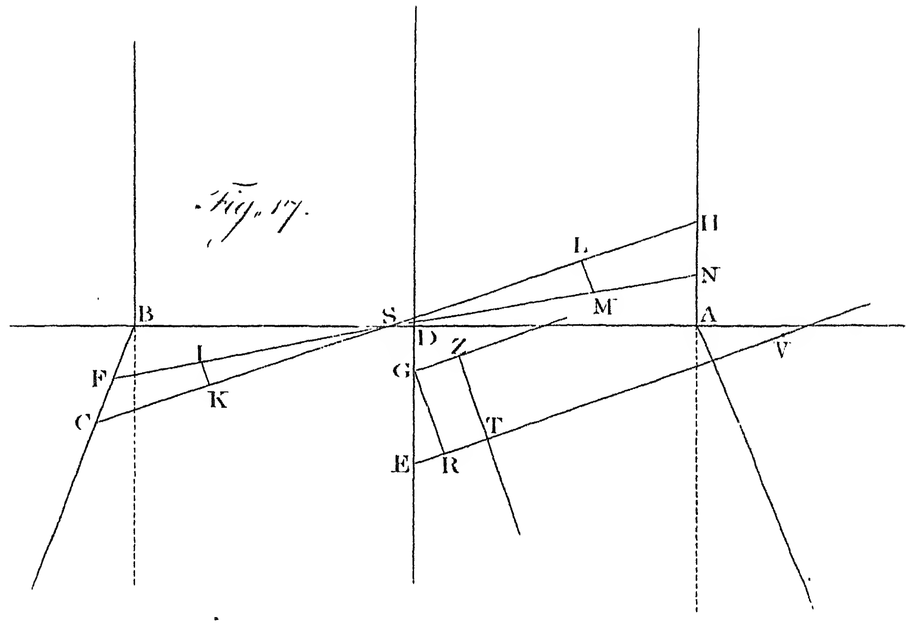

Fig. 17.

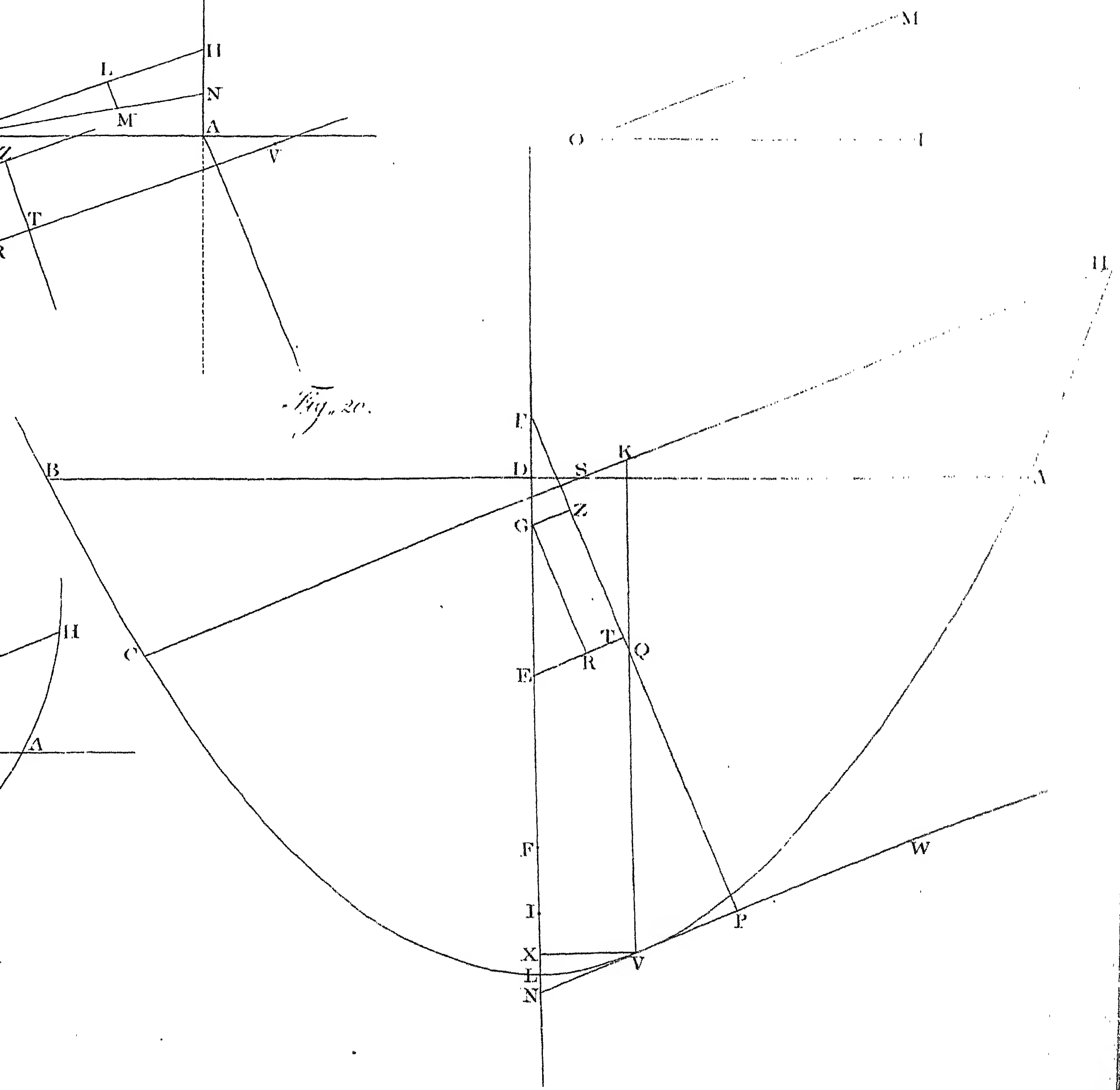

Fig. 20.

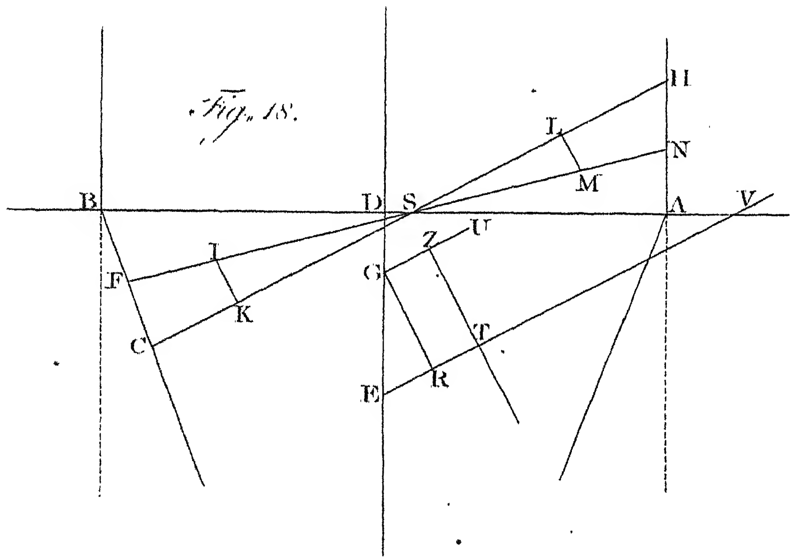

Fig. 18.

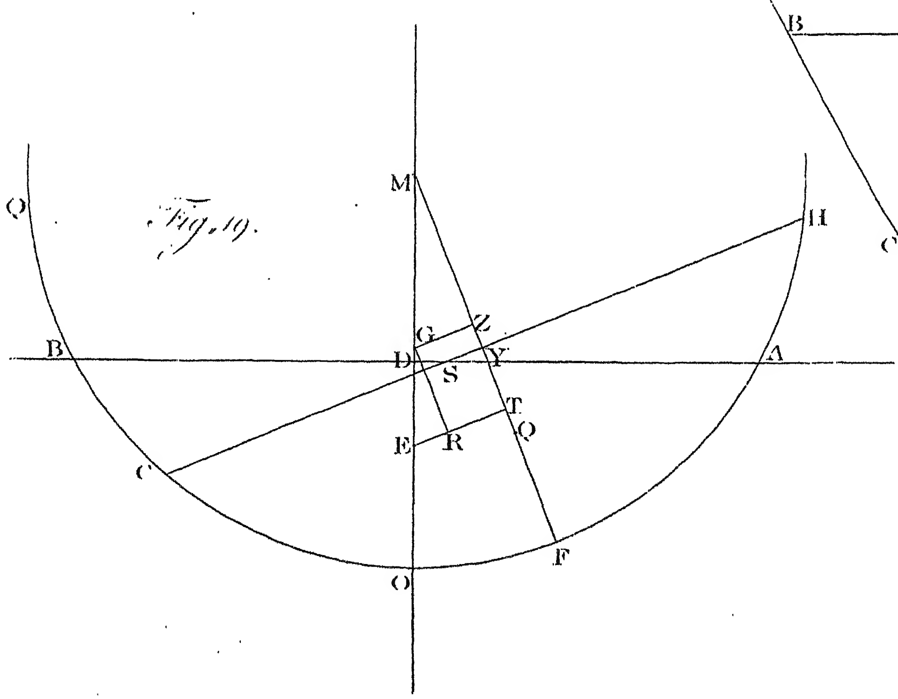

Fig. 19.

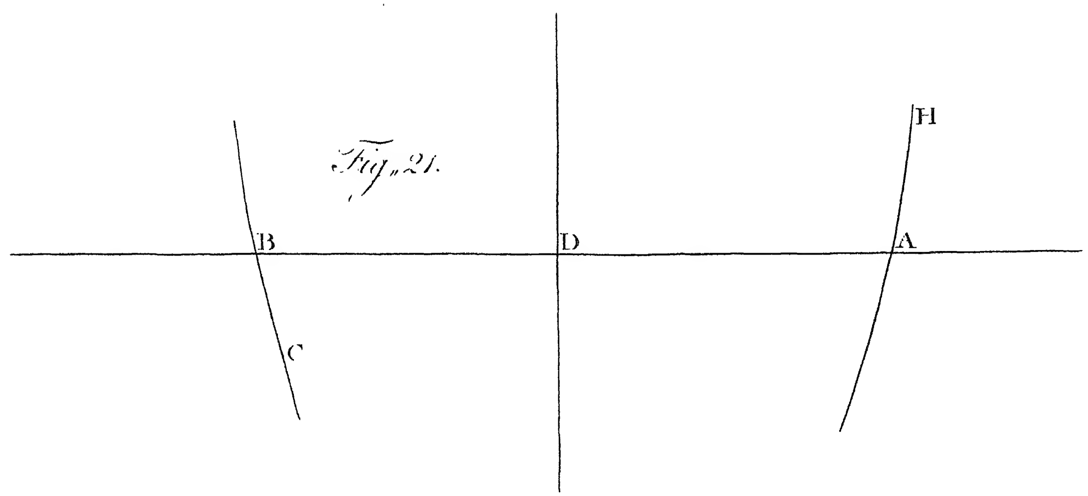

Fig. 21.

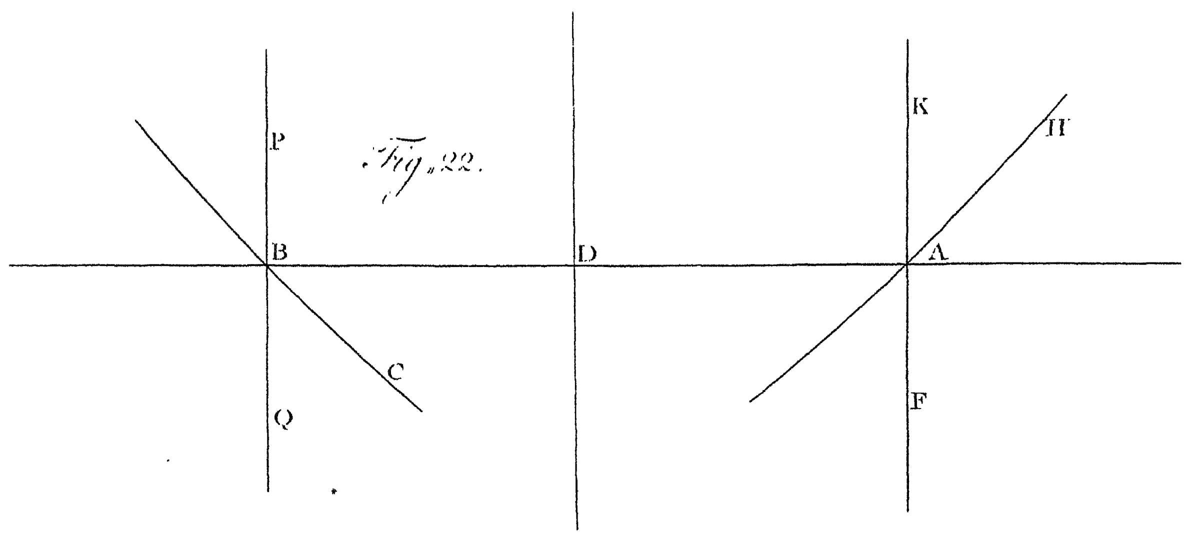

Fig. 22.

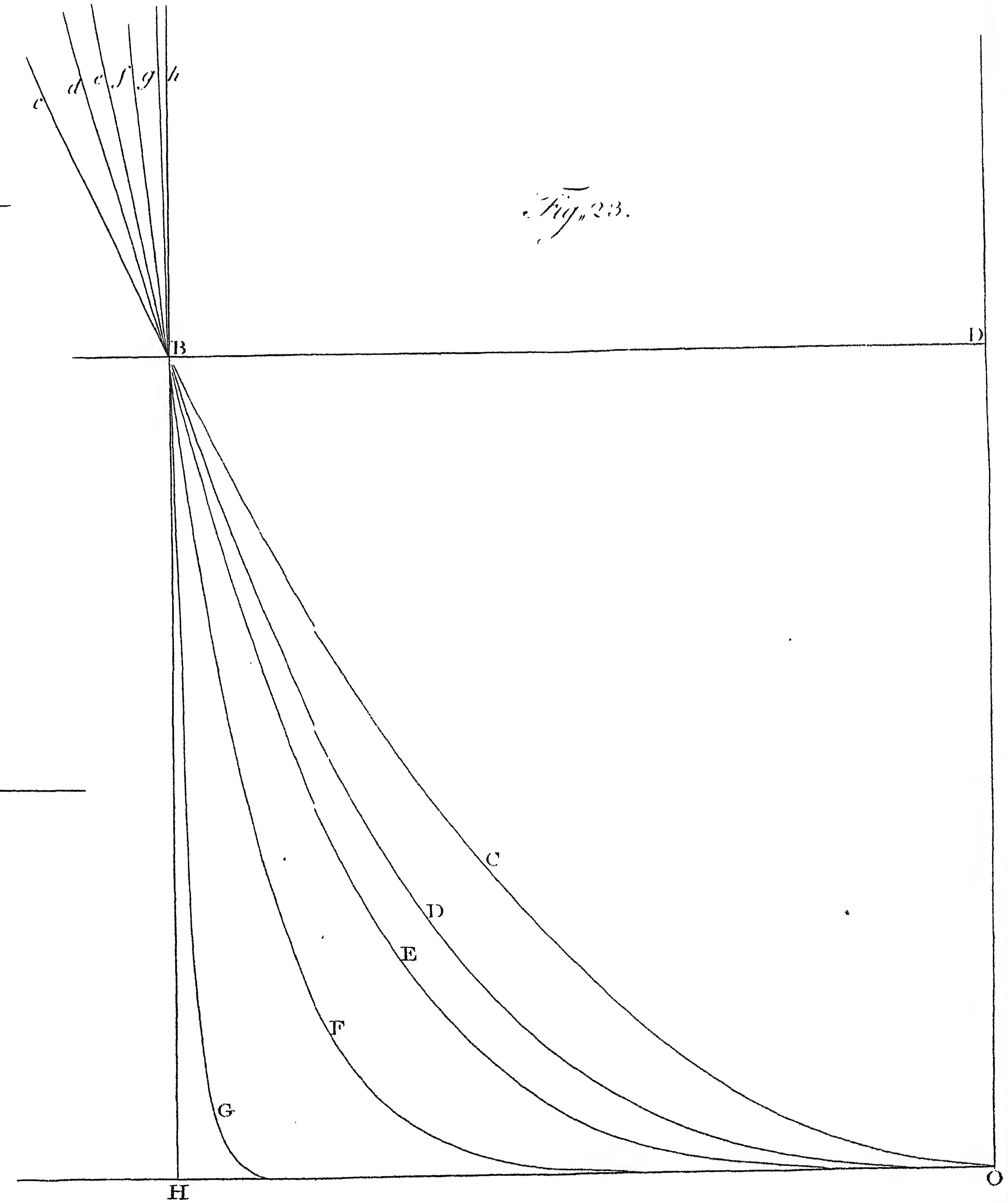

Fig. 23.

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Fig. 24.

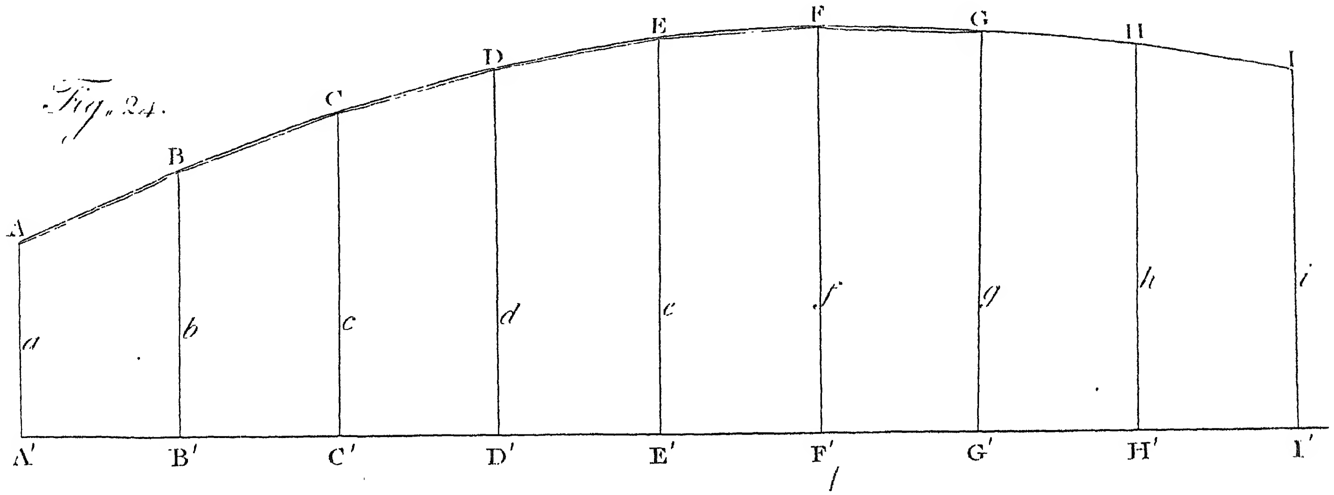


Fig. 25. Fig. 26.

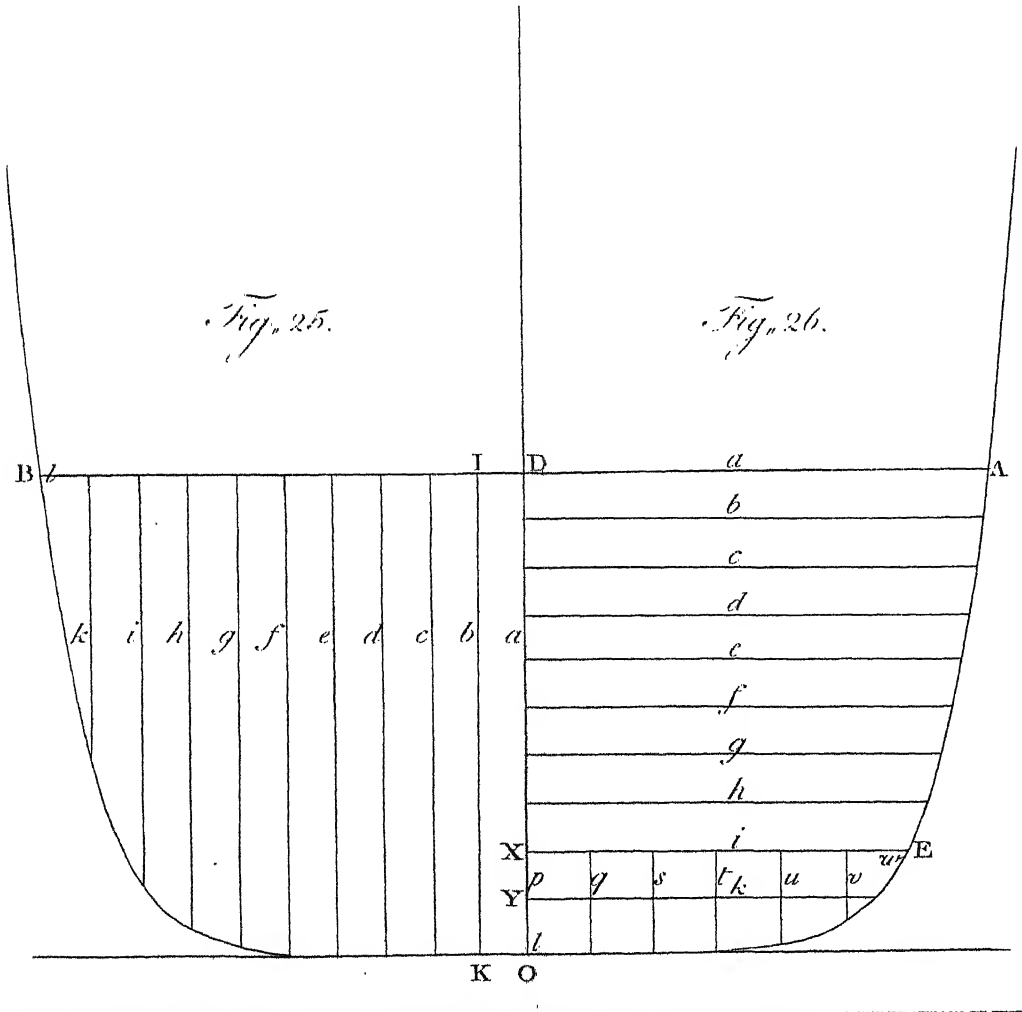
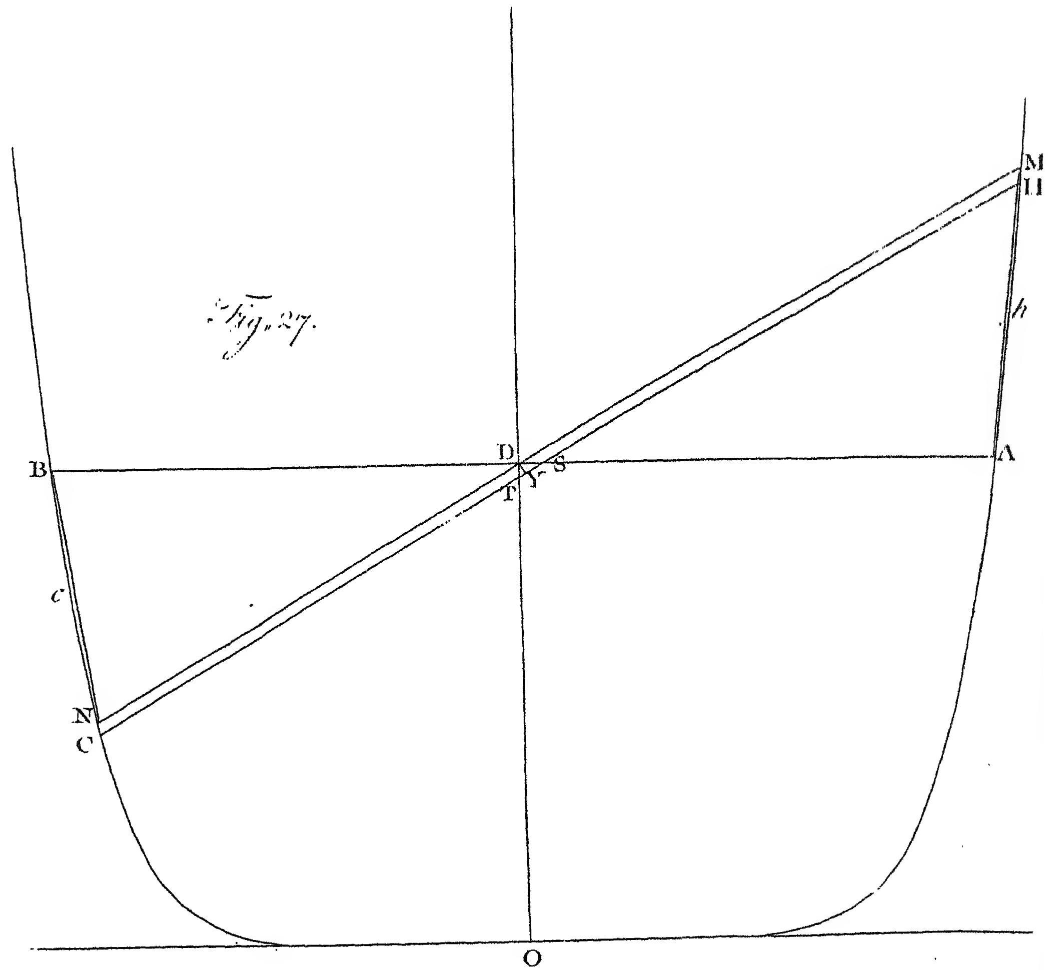


Fig. 27.



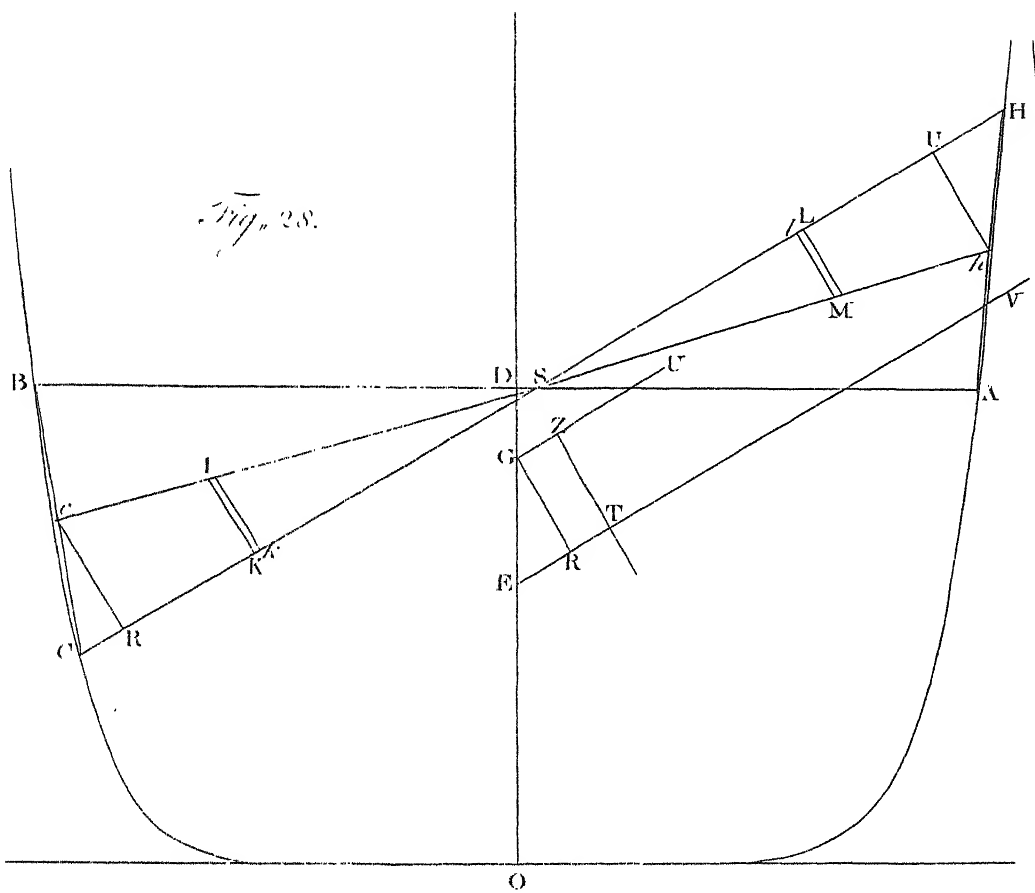

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Fig. 28.

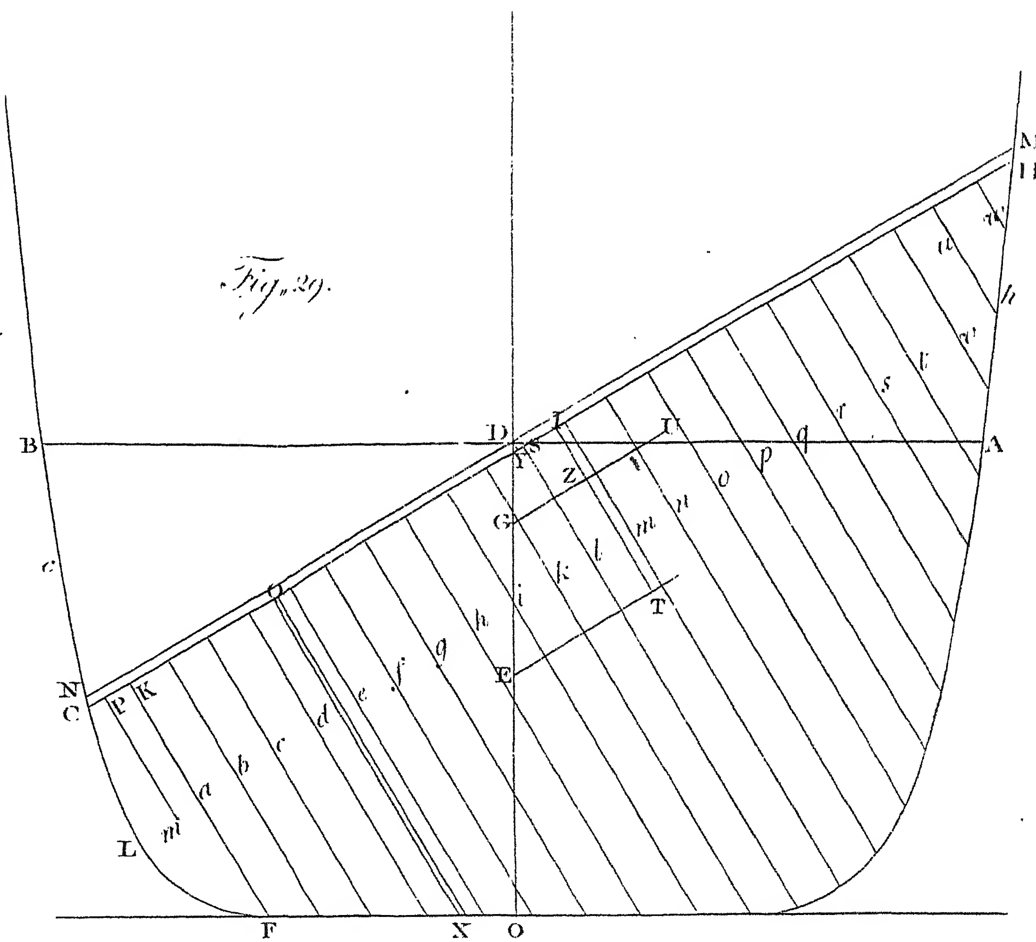
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Fig. 29.

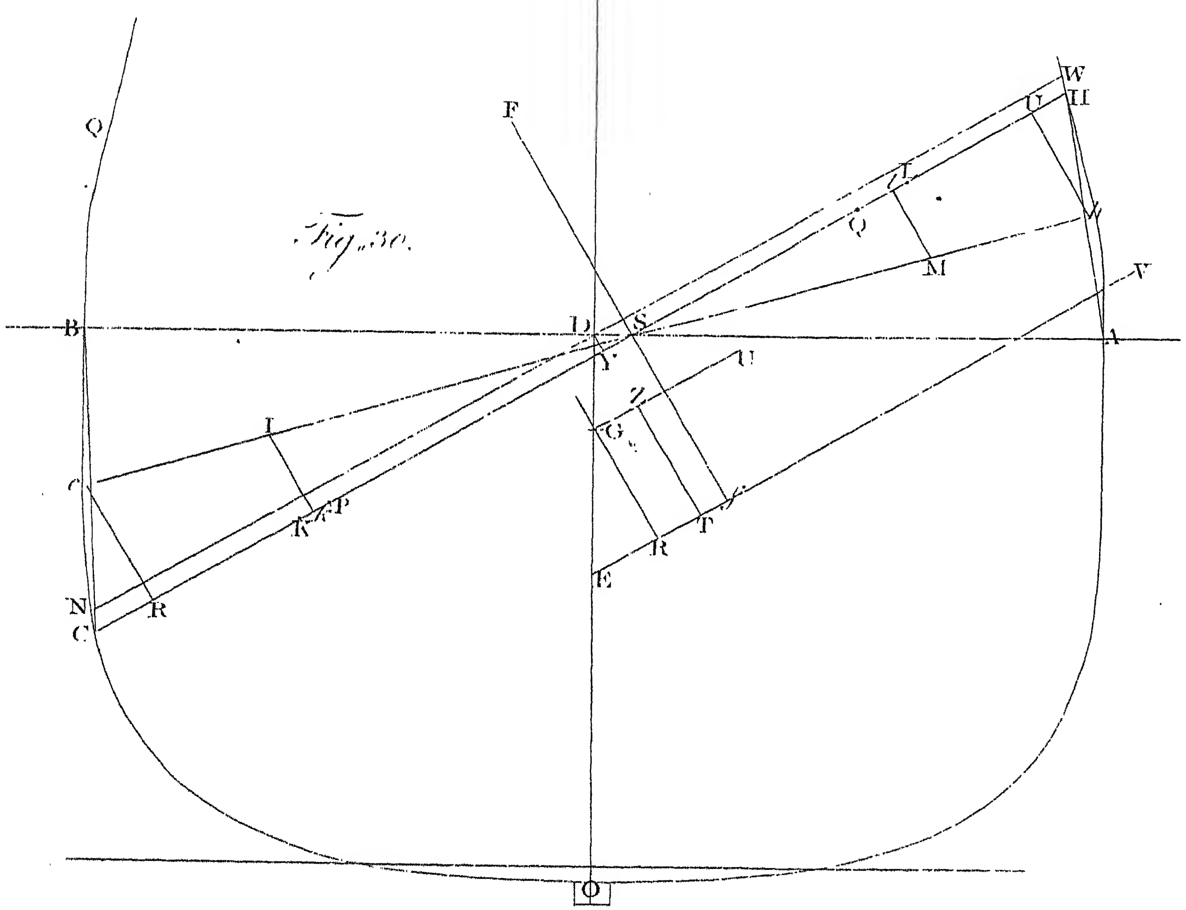


Fig. 30.

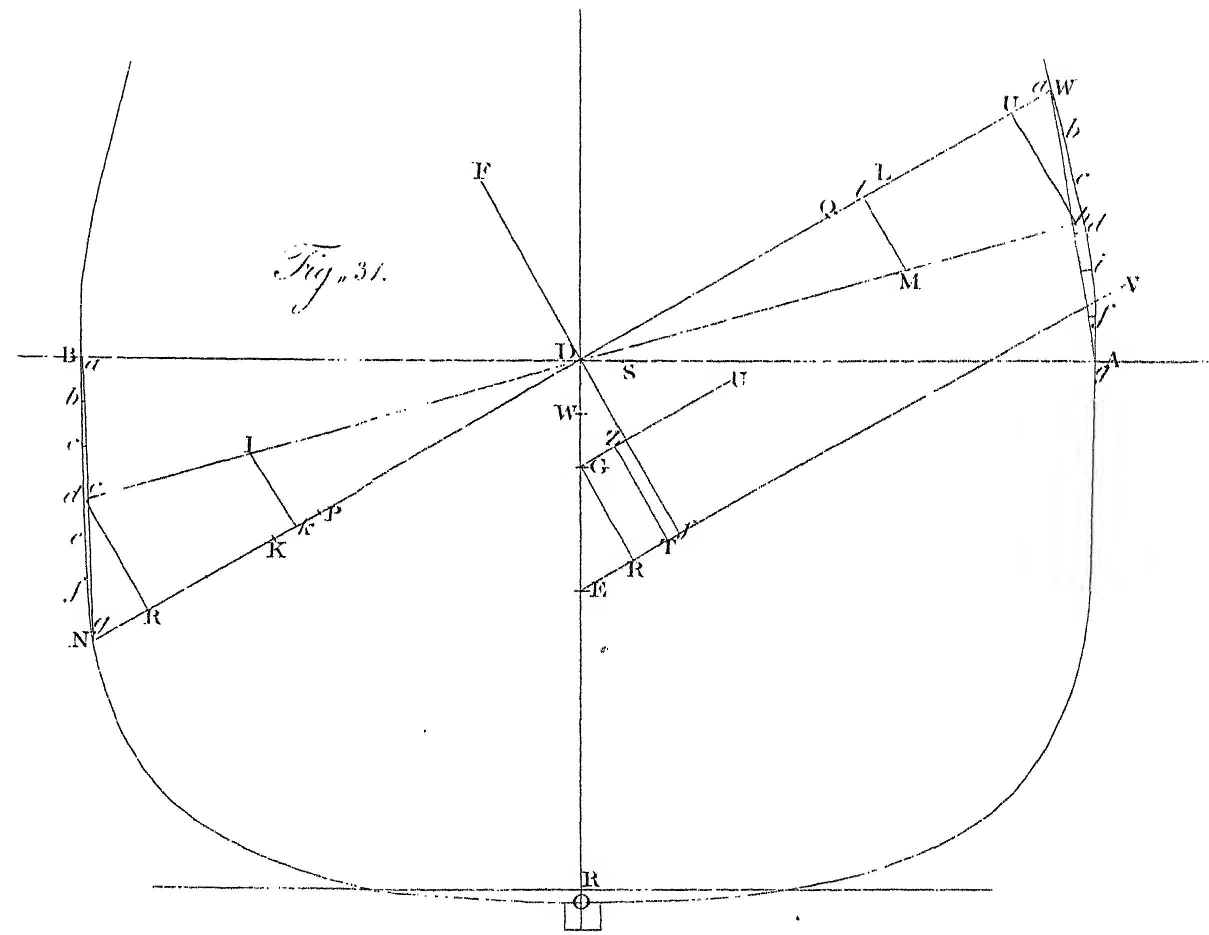


Fig. 31.

Note to the Investigation, Page 233.

$$WQ = FE \times \frac{\text{tang.}^2 \tfrac{1}{2} F \times \text{tang. } S \times \text{sec. } S}{\sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S}}$$

The investigation follows:

$FP : PO :: \text{sin. } O : \text{sin. } F$; wherefore $OP = FP \times \frac{\text{sin. } F}{\text{sin. } O}$; or because $FP^{*} = FE \times \text{sec. } \tfrac{1}{2} F \times \sqrt{\frac{\text{sin. } O}{\text{sin. } P}}$, $OP = FE \times \frac{\text{sec. } \tfrac{1}{2} F \times \text{sin. } F}{\sqrt{\text{sin. } O \times \text{sin. } P}}$ $= FE \times \frac{2 \text{ sin. } \tfrac{1}{2} F}{\sqrt{\text{sin. } O \times \text{sin. } P}}$, and $\frac{OP}{2} = PQ = FE \times \frac{\text{sin. } \tfrac{1}{2} F}{\sqrt{\text{sin. } O \times \text{sin. } P}}$; or because $\sqrt{\text{sin. } O \times \text{sin. } P}\,\dagger = \frac{\sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S}}{\text{sec. } \tfrac{1}{2} F \times \text{sec. } S}$, $PQ = FE \times \frac{\text{tang. } \tfrac{1}{2} F \times \text{sec. } S}{\sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang. } S}}$.

But $PW : WF \left(= FE \times \ddagger \sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S}\right) :: \text{sin. } \tfrac{1}{2} F : \text{sin. } P$; therefore $PW = FE \times \text{sin. } \tfrac{1}{2} F \times \frac{\sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S}}{\text{sin. } P}$ and $WQ = PW - PQ = FE \times \frac{\sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S} \times \text{sin. } \tfrac{1}{2} F}{\text{sin. } P}$ $- FE \times \frac{\text{tang. } \tfrac{1}{2} F \times \text{sec. } S}{\sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S}}$ or

$$WQ = FE \times \frac{\text{sin. } \tfrac{1}{2} F - \text{tang.}^2 \tfrac{1}{2} F \times \text{sin. } \tfrac{1}{2} F \times \text{tang.}^2 S - \text{tang. } \tfrac{1}{2} F \times \text{sec. } S \times \text{sin. } P}{\text{sin. } P \times \sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S}}$$

But $\text{sin. } P = \text{cos. } \overline{S + \tfrac{1}{2} F} = \text{cos. } \tfrac{1}{2} F \times \text{cos. } S - \text{sin. } \tfrac{1}{2} F \times \text{sin. } S$, which being substituted for sin. P in the value of WQ just found, the result is

$$WQ = FE \times \frac{\text{sin. } \tfrac{1}{2} F - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S \times \text{sin. } \tfrac{1}{2} F - \text{sin. } \tfrac{1}{2} F + \text{tang. } \tfrac{1}{2} F \times \text{sin. } \tfrac{1}{2} F \times \text{tang. } S}{\text{cos. } \tfrac{1}{2} F \times \text{cos. } S - \text{sin. } \tfrac{1}{2} F \times \text{sin. } S \times \sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S}}$$

$$\text{or } WQ = FE \times \frac{\text{tang. } \tfrac{1}{2} F \times \text{sin. } \tfrac{1}{2} F \times \text{tang. } S - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S \times \text{sin. } \tfrac{1}{2} F}{\text{cos. } \tfrac{1}{2} F \times \text{cos. } S - \text{sin. } \tfrac{1}{2} F \times \text{sin. } S \times \sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S}}$$

$$\text{or } WQ = FE \times \frac{\text{tang. } \tfrac{1}{2} F \times \text{sin. } \tfrac{1}{2} F \times \text{tang. } S \times \overline{1 - \text{tang. } \tfrac{1}{2} F \times \text{tang. } S}}{\text{cos. } \tfrac{1}{2} F \times \text{cos. } S - \text{sin. } \tfrac{1}{2} F \times \text{sin. } S \times \sqrt{1 - \text{tang.}^2 \tfrac{1}{2} F \times \text{tang.}^2 S}}, \text{ or}$$

* Demonstrated in page 232. † Page 308. ‡ Page 233.

because $\cos.\frac{1}{2}F \times \cos.S - \sin.\frac{1}{2}F \times \sin.S = \cos.\frac{1}{2}F \times \cos.S \times \overline{1 - \text{tang}.\frac{1}{2}F \times \text{tang}.S}$

$$WQ = FE \times \frac{\text{tang}.\frac{1}{2}F \times \sin.\frac{1}{2}F \times \text{tang}.S \times \overline{1 - \text{tang}.\frac{1}{2}F \times \text{tang}.S}}{\cos.\frac{1}{2}F \times \cos.S \times \overline{1 - \text{tang}.\frac{1}{2}F \times \text{tang}.S} \times \sqrt{1 - \text{tang}.^2\frac{1}{2}F \times \text{tang}.^2 S}},$$

or $WQ = FE \times \dfrac{\text{tang}.^2\frac{1}{2}F \times \text{tang}.S \times \sec.S}{\sqrt{1 - \text{tang}.^2\frac{1}{2}F \times \text{tang}.^2 S}}$.

The work of Mr. CHAPMAN, mentioned in page 267, is written in the Swedish language, and has been translated into French by M. VIAL DE CLAIRBOIS. The translation is intitled, *Traité de la Construction des Vaisseaux*, &c. *par* FREDERIC HENRI DE CHAPMAN, *Premier Constructeur des Armées Navales*, &c.

[To face page 310.

Dimensions of the CUFFNELLS INDIA SHIP.

The Ordinates entered in the annexed Table are the Half-breadths, expressed in Feet, of the several vertical and horizontal Sections.

VERTICAL SECTIONS.

HORIZONTAL SECTIONS.

N°	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17
	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
16	13.5	17.1	18.9	19.85	20.4	20.55	20.65	20.55	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5	20.5
15	13.0	17.0	18.9	20.0	20.55	20.75	20.9	21.0	21.0	21.0	21.0	21.0	21.0	21.0	21.0	21.0	21.0
14	12.4	16.75	18.8	20.05	26.6	20.9	21.1	21.2	21.3	21.3	21.3	21.3	21.3	21.3	21.3	21.3	21.3
13	11.6	16.4	18.6	20.0	20.16	20.105	21.15	21.3	21.4	21.45	21.45	21 45	21.45	21.45	21.45	21.45	21.45
12	10.78	16.0	18.4	19.85	20.54	20.94	21.2	21.38	21.48	21.5	21.56	21.56	21.56	21.56	21.56	21.55	21.53
11	9.6	15.35	18.1	19.7	20.5	20.92	21.21	21.44	21.55	21.60	21.60	21.60	21.60	21.60	21.60	21.60	21.60
10	8.1	14.4	17.7	19.45	20.3	20.8	21.15	21.38	21.52	21.60	21.60	21.60	21.60	21.60	21.60	21.60	21.60
9	6.6	13.25	17.075	19.1	20.075	20.65	21.05	21.34	21.45	21.5	21.59	21.59	21.59	21.59	21.59	21.59	21.55
8	4.9	11.73	16.24	18.55	19.73	20.45	20.9	21.2	21.34	21.47	21.51	21.51	21.51	21.51	21.51	21.51	21.50
7	3.3	10.0	15.1	17.84	19.3	20.12	20.65	20.9	21.14	21.28	21.51	21.51	21.51	21.35	21.35	21.32	21.3
6	1.7	8.2	13.5	16.85	18.64	19.65	20.32	20.62	20.78	20.93	20.93	21.1	21.1	21.1	21.1	21.05	21.0
5		6.2	11.3	15.3	17.55	18.91	19.60	20.05	20.25	20.4	20.5	20.6	20 6	20.60	20.6	20.52	20.45
4		4.09	9.0	13.2	15.09	17.7	18.65	19.05	19.35	19.53	19.6	19.75	19.75	19.75	19.75	19.65	19.55
3		1.8	6.35	10 45	13.5	15.6	16.8	17.45	17.88	18.15	18.3	18.3	18.3	18.3	18.3	18 20	18.05
2			3.5	6.85	9.78	12.15	13.6	14.55	15.2	15.5	15.9	15.9	15.9	15.9	15.9	15.7	15.5
1				2.55	3.09	5.5	6.75	8.	9.1	9.8	10.5	10.5	10.5	10 5	10.5	10.3	9.8

N°	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34
	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.	Feet.
16	20.5	20.4	20.3	20.2	20.15	19 85	19.75	19.5	19.3	19.1	18.9	18.5	18.05	17.5	16.95	16.25	15.5
15	21.0	20.9	20.8	20.65	20.5	20.3	20.1	19.9	19.7	19.45	19.3	18.8	18.4	17.9	17.35	16.65	15.7
14	21.3	21.25	21.1	21.0	20.85	20.65	20.45	20.25	20.0	19.7	19.4	19.05	18.7	18.2	17.6	16.9	15.8
13	21.45	21.4	21.25	21.15	21.0	20.8	20.6	20.4	20.1	19.8	19.5	19.25	18.75	18.35	17.6	16.9	15.5
12	21.51	21.48	21 32	21.22	21.05	20.82	20.61	20.44	20.15	19.85	19.58	19.25	18.77	18.21	17.52	16.5	12.95
11	21.52	21.50	21.35	21 24	21.075	20.82	20.61	20.43	20.12	19.8	19.52	19.15	18.62	17.9	16.9	14.9	7.
10	21.55	21.50	21.35	21.25	21.05	20.83	20.61	20.4	20.1	19.75	19.44	19.	18.4	17.4	15.65	14.5	3.4
9	21.52	21.44	21.3	21 18	21.0	20.75	20.55	20.3	20.0	19.59	19.22	18.55	17.82	16.3	13.4	7.45	1.95
8	21.4	21.3	21.2	21.05	20 89	20.65	20.4	20.1	19.73	19.3	18.8	17.9	16.8	14.6	10.35	4.31	1.35
7	21.2	21.1	21.0	21.85	20.68	20.4	20.15	19.85	19.35	18.8	18.1	16.85	15.05	12.0	7.1	2.7	1.05
6	20.9	20.81	20.67	20.5	20.32	20.05	19.75	19.3	18.8	18.05	17.11	15.35	12.9	9.1	4.55	1.85	.90
5	20.35	20.25	20.1	19.9	19.7	19.3	19.0	18.46	17.88	16.8	15.45	13.10	10.1	6.12	2.9	1.32	.7 5
4	19.45	19.3	19.15	18.9	18.62	18.28	17.75	17.1	16.15	14.8	12.85	10.1	7.05	3.75	1.9	1.0	.70
3	17.95	17.75	17.52	17 25	16.9	16.40	15.7	14.8	13.4	11.6	9.35	6.65	4.25	2.3	1.25	.80	.65
2	15.35	15.0	14.6	14.2	13.62	12.9	11.9	10.8	9.2	7.2	5.38	3.55	2.4	1.45	.98	.75	.63
1	9.2	8.5	8.	7.2	6.4	5.9	5.1	4.2	3.35	2.5	1.8	1.40	1.11	.90	.80	.62	.60

Common distance between the vertical sections, 5 feet.
Common distance between the water-lines or horizontal sections, 2 feet.
Distance between the horizontal section 1, and the upper surface of the keel, $\frac{3}{4}$ of a foot.
The ordinates of the horizontal section 12 coincide with the water's surface when the vessel is loaded.

XI. *Quelques Remarques d'Optique, principalement rélatives à la Réflexibilité des Rayons de la Lumiere. Par* P. Prevost, *Professeur de Philosophie à Geneve, de l'Académie de Berlin, de la Société des Curieux de la Nature, et de la Société Royale d'Edimbourg. Communicated by Sir* Charles Blagden, *Knt. F. R. S.*

Read March 22, 1798.

PREMIERE PARTIE.

De la Réflexibilité.

§ 1. Le mot *réflexibilité* se prend en deux sens différens.

1. Newton (*Opt. l.* 1. *part* 1. *prop* 3.) entend par là, cette propriété d'un rayon de lumière homogène, en vertu de laquelle ce rayon est réfléchi, s'il tombe sous un certain angle d'incidence; et transmis, s'il tombe sous un angle plus petit: ou, plus simplement, une disposition à être réfléchi, et non transmis, à la limite qui sépare deux milieux réfringens. (*Opt. l.* 1. *part* 1. *defin.* 3.)

Ce philosophe pense qu'en ce sens la réflexibilité des rayons n'est pas la même. Il établit, par des expériences, qu'il estime concluantes, que les rayons plus réfrangibles sont aussi plus réflexibles. En sorte que, selon lui, toutes les circonstances étant données et constantes, si un rayon blanc tombe sous un certain angle sur la surface dirimante, le rayon violet sera réfléchi, tandis que les six autres seront encore transmis et réfractés. Mais, en augmentant l'angle d'incidence, on obtiendra

successivement la réflexion de tous les rayons, depuis le violet, qui est le plus réflexible, jusqu'au rouge, qui l'est le moins.

M. Brougham (*Trans. Phil. pour* 1796, *part I. p.* 272.) ne trouve pas concluantes les expériences par lesquelles Newton établit cette proposition; et, fondé sur une autre expérience, il établit la proposition contraire, savoir, que tous les rayons ont la même disposition à être réfléchis, pourvu que l'angle d'incidence soit le même.

2. M. Brougham entend par *réflexibilité,* une disposition à être réfléchi près de la perpendiculaire à un certain dégré: en d'autres termes, une propriété du rayon homogène, par laquelle son angle de réflexion est à l'angle d'incidence en un certain rapport, qui n'est pas celui d'égalité, si ce n'est en quelques cas qu'il indique.

Selon ce physicien, ce rapport varie pour chaque rayon homogène. Le rapport d'égalité a lieu pour les rayons qui confinent au bleu et au vert: le rapport d'inégalité a lieu pour les autres; et les plus réfrangibles sont le moins réflexibles. En sorte que, pour le rayon rouge, l'angle de réflexion est moindre, et pour le violet, plus grand, que l'angle d'incidence.

On sait que Newton affirme, au contraire, que l'angle de réflexion est *toujours* égal à l'angle d'incidence.

Discutons ces sentimens opposés.

§ 2. Premiere question. Les rayons homogènes diffèrent-ils en réflexibilité au sens Newtonien? En d'autres termes, sous un même angle d'incidence, arrive-t-il que le rayon violet soit réfléchi, tandis que le rouge ne l'est pas; toutes choses d'ailleurs étant précisément pareilles?

Des deux expériences, par lesquelles Newton établit l'inégale réflexibilité des rayons, il suffira de rappeller celle que M. Brougham attaque directement.

Newton fit tomber un rayon blanc perpendiculairement à la face antérieure d'un prisme: puis, tournant le prisme sur son axe, il observoit la réflexion qui s'opéroit à sa face postérieure. Il vit le violet se réfléchir le premier; puis les autres rayons, dans l'ordre de leurs réfrangibilités, jusqu'au rouge, qui fut réfléchi le dernier. Il en conclut, que le violet est réfléchi sous un moindre angle d'incidence que le rouge. (Expér. 9.)

C'est cette conclusion que M. Brougham attaque; et, pour ne point altérer sa pensée, je vais transcrire ici ses expressions.

"That the demonstration involves a logical error, appears "pretty evident. When the rays, by refraction through the "base of the prism used in the experiment, are separated into "their parts, these become divergent, the violet and red emer-"ging at very different angles, and these were also incident on "the base at different angles, from the refraction of the side "at which they entered; when, therefore, the prism is moved "round on its axis, as described in the proposition, the base is "nearest the violet, from the position of the rays by refraction, "and meets it first; so that the violet being reflected as soon "as it meets the base, it is reflected before any of the other "rays, not from a different disposition to be so, but merely "from its different refrangibility."

Ainsi M. Brougham pense que la réflexion du rayon violet ne précéde celle du rayon rouge, que parceque la réfraction qui a lieu à la surface antérieure, force le rayon violet à atteindre la surface postérieure *plutôt* que ne peut le faire le rouge.

Mais il semble que l'effet est ici en sens inverse de la cause. Ecartons d'abord un faux sens. Il est impossible que l'auteur

veuille dire que l'oeil peut saisir l'intervalle de temps qui s'écoule entre l'arrivée du rayon violet, et celle du rayon rouge, à la face postérieure du prisme. Maintenant, celui des deux rayons qui décrit la route la plus courte, tombe plus près de la perpendiculaire abaissée du point de départ; et de cela seul on peut conclure, qu'il tombe sous un angle d'incidence plus petit. D'où il suit, que c'est le rayon rouge qui devroit être réfléchi le premier, et non le violet.

En effet, considérons d'abord la position du prisme au premier moment, et telle que la réprésente la figure que NEWTON en a donnée dans son *Optique*. Le rayon blanc FM (Tab. XVI. fig. 1.) est perpendiculaire sur AC: en ce cas, il n'est pas réfracté à son immergence, et suit la droite FM. A ce point, NEWTON réprésente le seul rayon violet, MN, réfléchi, tandis que tous les autres, tels que MH, MI, sont transmis et réfractés; (du moins les seuls violets sont réfléchis en entier.)

Cependant il est certain qu'il a fallu, pour obtenir ce phénomène, chercher, en faisant tourner le prisme, à lui donner le dégré d'inclinaison qui pouvoit faire réussir l'expérience: et M. BROUGHAM a raison d'observer, que dès lors le perpendicularisme du rayon, sur la face antérieure, AC, a dû cesser; qu'en conséquence il y a eu réfraction, et que les divers rayons homogènes n'ont point suivi une route réctiligne, telle que FM, et n'ont point rencontré la face postérieure, BC, sous des angles égaux.

Soit donc maintenant A'B'C' (fig. 2.) la nouvelle position du prisme, qu'il a pris en vertu de sa rotation sur son axe: alors le rayon FP tombera obliquement sur A'C', au point P; de sorte que la perpendiculaire PO sera du côté A', par conséquent comprise dans l'angle A'PF. C'est ce qui résulte, 1°. du

but que s'est proposé NEWTON, savoir, d'augmenter l'angle d'incidence sur la face postérieure; lequel angle (formé au point M, dans la fig. 1. qui répresente la première position du prisme,) étoit trop petit pour produire la réflexion. 2°. Des expressions précises de NEWTON, qui dit que *le prisme* ABC *est tourné sur son axe, selon le sens qu'indique l'ordre des lettres* A, B, C, dans sa figure, qui, pour l'objet que j'ai en vue, est la même que ma fig. 1.

Le rayon FP (fig. 2.) sera donc réfracté en s'approchant de la perpendiculaire OP: mais le rayon le plus réfrangible (le violet) s'en approchera le plus; le moins réfrangible (le rouge) s'en approchera le moins. Ainsi les routes que suivront ces rayons sont bien représentées par les lignes PV, PR. Le rayon violet fera donc, avec la face postérieure B′C′, un angle PVC′, plus grand que l'angle PRC′, formé par le rouge. Or, les angles d'incidence, aux points V, R, sont les complémens des angles PVC′, PRC′, respectivement.

Il est donc certain, qu'en vertu de la réfraction qui s'opère à la face antérieure, le rayon violet rencontre la postérieure sous un angle d'incidence moindre que le rouge; et, par conséquent, le premier est dans des circonstances plus défavorables à la réflexion que le second; cependant, le premier est réfléchi, tandis que le second ne l'est pas encore. On est donc en droit de conclure que, par sa nature, il est plus réflexible au sens NEWTONIEN.*

* Tout ceci s'applique également à la 10ème Expérience de NEWTON, dans laquelle il emploie deux prismes, réunis en un seul parallélipipède. Dans l'une et l'autre Expérience, (*Expér.* 9 *et* 10.) il est question d'un autre prisme, destiné à rendre l'effet plus sensible, en dispersant les rayons réfléchis: il étoit inutile de parler ici de cet accessoire.

Ainsi la considération introduite par M. BROUGHAM (et qui est très juste) fait conclure *a fortiori* en faveur de l'assertion NEWTONIENNE. On peut dire, que non seulement, à même incidence, le violet se réfléchit, tandis que le rouge ne se réfléchit pas; mais même on doit dire, que ce phénomène a lieu, quoique l'incidence du violet soit plus défavorable à la réflexion que celle du rouge.

Donc enfin les rayons diffèrent en réflexibilité au sens de NEWTON; et le plus réfrangible est aussi le plus réflexible.

Jusqu'ici, pour rendre mon raisonnement plus simple, j'ai laissé indéterminé l'angle réfringent, C, du prisme. NEWTON le détermine. Dans l'*Expérience* 9. *du liv.* 1. *part* 1. *de son Optique,* il employoit un angle réfringent de 45°; et cependant il dit expressément, que les rayons entroient *perpendiculairement:* d'où il suit, que l'angle d'incidence, au point M, étoit aussi de 45°. On seroit donc fondé à croire, que des rayons tombant sous cette incidence sur la face BC, et passant du verre à l'air, ne sont pas tous réfléchis. C'est ce qu'affirme M. BRISSON, en exposant cette même expérience (*Traité élem. ou Princ. de Phys. Paris,* 1789. *T. II.* §. 1411.) Cependant il est bien connu que la réflexion totale a lieu sous un angle moindre, savoir, aux environs de 40°. On le détermine même avec la plus grande précision: je citerai une seule autorité. "A ray "of light will not pass out of glass into air, if the angle of in- "cidence exceeds 40° 11'." (*Lectures on Nat. and Exper. Phil. by George Adams, London,* 1794, *Vol. II. p.* 163.) La détermination de NEWTON ne diffère pas sensiblement de celle-ci." "To- "talis reflexio tum incipit, cum angulus incidentiæ sit 40° 10'." (*Opt. l.* 2. *p.* 3. *prop.* 1.) Dirons-nous que, sous l'angle de 45°, il passoit encore quelques rayons, suffisans pour rendre l'expé-

rience sensible? Dirons-nous que le rayon FM n'étoit pas exactement perpendiculaire sur la face AC? Je pense que cette dernière assertion est vraie; c'est-à-dire, que dans la première position du prisme, le rayon FM commençoit par être oblique sur AC, dans le sens opposé à celui qu'indique la fig. 2.; en sorte que l'angle APF étoit moindre que FPC. D'où il résultoit, que les rayons les plus réfrangibles tomboient sur la face postérieure BC, sous un angle d'incidence plus grand, et, par là-même, plus favorable à la réflexion. Sous cette forme, l'argument de M. BROUGHAM reprend une nouvelle force.

Ici l'*Optique* de NEWTON ne se suffit pas à elle-même; elle a besoin d'un commentaire. Le meilleur sera celui que nous offrent ses *Lectiones Opticæ* (Is. NEWTONI *Opuscul. Lausannæ et Genevæ*, 1744. *Tom. II. p.* 217—223.) Voici comment l'auteur y répond à notre doute. " Ne qua oriatur suspicio, quod " refractiones in superficiebus AC et AB, ad ingressum radio-" rum in prisma et egressum factæ, possint aliquid conducere " ad effectus hosce producendos, observare licet, quod effectus " iidem producuntur, cujuscunque magnitudinis statuatur an-" gulus ACB*: hoc est, quæcunque sit refractio superficiei AC. " . . . . Imo possis efficere, quod, cum colores partim reflectun-" tur . . . . et partim trajiciuntur . . . . radii perpendiculariter " incidant in AC, emergantque ex AB, et sic neutra super-" ficie refringantur, modo statuas angulum ACB esse grad. " 40 circiter, et iidem tamen effectus producentur." (P. 219.) Les auteurs les plus exacts n'ont pas omis cette circonstance. ROBERT SMITH, après avoir exposé l'Expérience ix. de l'*Optique*

* La texte porte ABC, par une erreur typographique manifeste. Ceci est d'ailleurs indifférent à l'objet que j'ai en vue. Je ne cite pas ce qui a rapport à l'égalité requise entre les angles B et C, parceque cela n'influe pas sur ma recherche actuelle.

de NEWTON, dit (ou fait dire à NEWTON même) "Je ne me "suis pas apperçu ici d'aucune réfraction sur les côtés, AC, AB, "du premier prisme, parceque la lumière entroit *presque* perpen-"diculairement au premier côté, et sortoit *presque* perpendicu-"lairement au second, et par conséquent n'en souffroit aucune, "ou si peu que les angles d'incidence, à la base BC, n'en étoient "pas sensiblement altérés; *surtout, si les angles du prisme, à la "base* BC, *étoient chacun de* 40° *environ.* Car les rayons FM "commencent à être totalement réfléchis lorsque l'angle CMF "est d'environ 50°, et, par conséquent, ils formeront alors avec "AC un angle de 90°." (*Cours d'Optique de* ROBERT SMITH, *trad. par* L. P. P. [PEZENAS] *Tom. I.* §. 173. *p.* 190.)

Il résulte de tout ceci, que lorsque l'angle réfringent du prisme est bien choisi, un rayon blanc, perpendiculaire à sa face antérieure, AC, peut être décomposé, parcequ'il est réfléchi en partie, et non en totalité; le rayon violet étant réfléchi, tandis que le rouge est encore transmis.

Je remarquerai ici, que pour répéter cette expérience, et la rendre concluante, il n'est pas nécessaire de circonscrire l'angle C dans les limites qui rendent le rayon FM perpendiculaire (ou à peu près perpendiculaire) sur la face antérieure AC. Tous les raisonnemens que nous avons faits ci-dessus (§. 2.) seront justes, pourvû que, dans la première position du prisme, l'angle APF soit plus grand que son angle de suite FPC; or, pour que cette circonstance ait lieu, lorsque la réflexion s'opère au point M, il suffit que l'angle réfringent, C, soit moindre que 40°.

Cette expérience variée pourroit offrir quelques résultats intéressans; mais les autorités que j'ai cité ne laissent pas de doute sur le résultat particulier que nous avions à examiner.

Les rayons plus réfrangibles sont réfléchis sous un angle d'incidence moindre. Ils sont plus réflexibles au sens NEWTONIEN.

§. 3. Mais M. BROUGHAM étaye l'opinion contraire, d'une expérience qu'il énonce ainsi :

"I held a prism vertically, and let the spectrum of another "prism be reflected by the base of the former, so that the rays "had all the same angle of incidence; then, turning round the "vertical prism on its axis, when one sort of rays was trans-"mitted or reflected, all were transmitted or reflected."*

La discussion complette de cette expérience exigeant d'assez longs détails, je me contenterai d'observer, que le plan de la face verticale, sur lequel s'opéroit la réfraction, ne pouvoit point être ajusté de manière à produire un même angle d'incidence avec tous les rayons du spectre à la fois : et, supposant même la chose possible, et exécutée un instant, la rotation du prisme eût changé cette disposition, en altérant inégalement cet angle, pour divers rayons. On peut concevoir, en conséquence, une multitude de résultats divers; entr'autres, on peut concevoir que les angles d'incidence des divers rayons soient tels, que l'observation de M. BROUGHAM se concilie avec le sentiment de NEWTON, sur leur inégale réflexibilité. Mais, puisque M. BROUGHAM n'entre pas dans ce détail, et ne donne qu'un résultat unique, il est à présumer qu'il n'a pas répété, ou du moins varié, cette expérience. Ce physicien paroît même n'y pas donner beaucoup d'importance, par la manière rapide dont il l'énonce.

* La même expérience, tentée par NEWTON, lui à donné précisément le résultat contraire. "Radii purpuriformes primo omnium reflectuntur, et ultimo rubriformes." *Lect. Opt. Opuscul. Tom. II. p. 220.*

Je pense donc, qu'elle ne peut pas, quant à présent, infirmer les conclusions de NEWTON ; et qu'on est encore en droit d'affirmer, au sens de ce philosophe, que les rayons les plus réfrangibles sont aussi les plus réflexibles.

§. 4. SECONDE QUESTION. Les rayons homogènes diffèrent-ils en réflexibilité au sens BROUGHAMIEN ? En d'autres termes, sous un même angle d'incidence, le rayon rouge forme-t-il un angle de réflexion moindre, et le violet un angle de réflexion plus grand, que l'angle d'incidence ?

§. 5. L'expérience fondamentale de laquelle M. BROUGHAM déduit cette inégale réflexibilité, au sens de sa définition, est celle-ci.

Un cylindre brillant et poli, d'un très petit diamétre, (une fibre métallique,) étant présenté par sa convexité à un rayon blanc, a réfléchi un spectre coloré ; et, tout étant mesuré ou calculé convenablement, il a paru que les rayons qui confinent au bleu et au vert étoient les seuls qui fussent réfléchis sous un angle égal à l'angle d'incidence. Les rouges étoient réfléchis sous un angle moindre ; les violets sous un plus grand.

Maintenant la question se réduit à savoir si cette expérience est concluante en faveur de la thèse de M. BROUGHAM.

§. 6. Pour s'en assurer, il est important de rappeller un principe posé par NEWTON, et admis par M. BROUGHAM ; (p. 250.) c'est que la force quelconque qui produit la réflexion, agit selon une ligne perpendiculaire à la surface réfléchissante.

§. 7. De ce principe il suit, que la réflexion opérée par une surface plane, doit se faire sous la loi admise jusqu'à présent par tous les opticiens. (NEWTON, *Princip. l.* 1. *prop.* 96.) Et cela est vrai, quelle que soit l'intensité de la force répulsive, et aussi, quelles que soient la vitesse et l'inclinaison du rayon in-

cident; pourvû que le rayon soit réellement incident, et ne se meuve pas parallélement à la surface répulsive.

§. 8. Cette conséquence, et toute la démonstration NEWTONIENNE qui l'établit, supposent que la surface agit sur le rayon pendant toute sa route dans la sphère de son activité; et également à d'égales distances. M. BROUGHAM n'allégue rien contre cette hypothèse, il paroît l'admettre même expressément; (p. 269.) et, en effet, comment pourroit-on ne point l'admettre?

§. 9. Ainsi, d'après un principe qui n'est pas contesté, il paroît que la réflexion ne peut point décomposer la lumière blanche, lorsque celle-ci est réfléchie en totalité par une surface plane.

Ceci est parfaitement conforme à ce qu'observe M. BROUGHAM, que, par aucun moyen, on ne peut réussir à opérer cette décomposition, en employant des surfaces planes, ni des surfaces courbes d'un rayon qui ne soit pas très petit, et, pour ainsi dire, évanouissant. On conçoit, en effet, qu'un élément de surface courbe d'un rayon plus grand, est un vrai plan, pour une particule de lumière. L'auteur, à la vérité, explique ce phénomène d'une autre manière; mais le fait, indépendamment de toute explication, n'en est pas moins certain et reconnu.

§. 10. Soit maintenant HHH (fig. 3.) un très petit cylindre brillant et poli, (une fibre métallique;) et BRVK le cylindre, sur même axe, qui est la sphère d'activité de ce petit corps: l'un et l'autre représentés par leur section circulaire. (Ces deux cercles, fort inégaux, se confondent à l'observation.)

AB représente un rayon blanc, incident, au point B, sur le cylindre, ou sur sa sphère d'activité.

Supposons les rayons homogènes inégalement répulsifs, et

que le rouge soit plus repoussé que le violet : c'est une supposition qu'admet M. BROUGHAM. (P. 267.)

Dans cette hypothèse, le rayon violet devra pénétrer plus profondément dans la sphère répulsive.

La route que décrit un rayon homogène, dans l'intérieur de la sphère d'activité répulsive, doit être formée de deux branches égales et semblables ; et l'axe doit passer par le centre de la sphère, ou de la section du cylindre. Cela résulte du principe posé ci-dessus. (§. 6.)

Et une conséquence immédiate de cette remarque, c'est que ce rayon homogène ressortira de la sphère d'activité, sous un angle de réflexion égal à l'angle d'incidence.

En sorte que tous les rayons homogènes, formant en B le même angle d'incidence, seront tous réfléchis sous des angles égaux.

Mais, puisque les uns se sont plus enfoncés que les autres dans la sphère d'activité, ils sortiront donc divergens ; car c'est le seul moyen de produire l'égalité des angles de réflexion.

La fig. 3. est destinée à exposer cet effet. Le rayon rouge, s'enfonçant moins dans la sphère, ou dans le cylindre d'activité BRVK, décrit la courbe BOR, dont l'axe passe par le centre C ; et il ressort par RG, faisant l'angle de réflexion ERG = ABD, angle d'incidence. Le rayon violet, s'enfonçant plus, décrit la courbe BQV, dont l'axe passe aussi par le centre C. Ce rayon ressort par VL, et les trois angles FVL, ERG, ABD sont égaux.

Mais l'observateur, voyant l'arc BRV comme un point, et sachant que les angles qu'il mesure sont la somme de l'angle d'incidence et de l'angle de réflexion, pour les rayons de chaque espèce, sera conduit à croire, que sous une même incidence

les angles de réflexion varient: car il trouvera que la droite AB forme, avec les droites RG, VL, des angles inégaux; et, en un mot, il aura toutes les mêmes apparences qui se sont offertes à M. Brougham. (§. 5.)

Il importe de remarquer ici, que si ce physicien affirme, que les rayons qui confinent au bleu et au vert sont réfléchis sous un angle de réflexion égal à l'angle d'incidence, ce n'est pas qu'il l'ait reconnu, ou pû reconnoître, par aucune expérience directe: mais c'est qu'il n'a eu aucune supposition plus naturelle à faire. Que si l'on prétendoit que tous les angles de réflexion sont plus petits, ou plus grands, que celui d'incidence; ou que la limite d'égalité tombe sur quelqu'autre division du spectre, on feroit une supposition gratuite, mais à laquelle l'observateur n'auroit aucun fait direct à opposer. Car il n'y a aucun moyen concevable, par lequel, dans ces expériences, l'angle d'incidence puisse être mesuré directement, et séparé de l'angle de réflexion.

§. 11. Puis donc, qu'en supposant (avec M. Brougham) que le rayon rouge est plus répulsif que le violet, on concilie parfaitement le phénomène observé avec la loi de réflexion reconnue pour les surfaces planes, il n'y a pas de raison de s'écarter de celle-ci.

Et je conclus, de tout ce qui précéde, que les rayons homogènes ne sont point inégalement réflexibles au sens Broughamien; en d'autres termes, que la loi de réflexion admise par Newton, est la vraie loi de la nature.

§. 12. Il résulte de la discussion précédente, que les rayons violets se réfléchissent plutôt, et les rouges plus fortement.

Lors même que ces deux effets auroient lieu dans des circonstances pareilles, ils ne seroient peut-être pas inconciliables.

On pourroit concevoir que la sphère d'activité s'étend un peu plus loin pour les violets que pour les rouges, mais qu'elle agit sur ceux-ci avec plus d'intensité.

Mais il est essentiel de faire remarquer, que ces deux effets ont lieu dans des circonstances très différentes, même opposées: et ceci indique une exception importante à l'assertion de NEWTON, sur l'inégale réflexibilité des divers rayons homogènes.

Dans les expériences par lesquelles ce physicien l'établit, (*Exp.* 9 *et* 10.) la réflexion s'opère dans le milieu le plus dense;* elle se fait donc par attraction. Au contraire, dans les expériences de M. BROUGHAM, (que j'ai exposées sommairement au §. 5.) la réflexion s'opère dans le milieu le plus rare: c'est-à-dire, qu'elle se fait par répulsion.

Ainsi, d'une part, on voit que les rayons les plus réfrangibles, ou les plus attirés dans l'acte de la transmission, sont aussi les plus attirés dans l'acte de la réflexion: et, d'autre part, on voit que les rayons les moins réfrangibles, ou les moins attirés dans la transmission, sont les plus repoussés (c'est-à-dire les moins attirés,) dans l'acte de la réflexion.

Ceci paroît faire exception à la loi d'inégale réflexibilité NEWTONIENNE; puisque cette inégale réflexibilité n'est prouvée par NEWTON, que pour le cas où le rayon se meut dans le milieu le plus dense. Je ne me rappelle pas que NEWTON, ni aucun opticien, jusqu'à M. BROUGHAM, ait traité l'autre cas. Les expériences de ce dernier physicien me paroissent indiquer (au

* Dans la 10ème Expérience, (la seule sur laquelle on pût élever un doute,) NEWTON dit bien, à la vérité, que la réflexion s'opère *par la base commune des deux prismes*, qu'il a réunis en un seul parallélipipède. Mais il me paroît que cette réunion ne pouvoit se faire, qu'en laissant entre les deux prismes une lame d'air, suffisante pour produire la réflexion à la surface antérieure. (*Opt. l.* 1. *part.* 1. *prop.* 3.)

moins indirectement) l'inégale réflexibilité NEWTONIENNE, pour le cas que NEWTON a négligé; je veux dire, pour celui où le rayon se meut dans le milieu le plus rare: et il en résulte, à ce qu'il me semble, quelque penchant à croire, que cette réflexibilité est en sens inverse de l'autre: ce qu'il étoit d'ailleurs assez naturel d'attendre.

SECONDE PARTIE.

Quelques Rapprochemens.

§. 13. PREMIERE QUESTION. Les principes qui expliquent la réflexion, expliquent-ils la flexion? *

Pour parvenir à répondre à cette question, il y a deux préliminaires indispensables: rappeller les principes qui ont été posés pour expliquer la réflexion; et indiquer avec précision les lois de la flexion.

§. 14. Les principes admis ci-dessus sont, 1°. la force répulsive agissant selon une direction perpendiculaire à la surface réfléchissante. 2°. Les rayons rouges plus répulsifs que les violets; ou, en général, les rayons les moins réfrangibles plus fortement repoussés que ceux qui sont plus réfrangibles.

§. 15. Les lois de la flexion, très bien déterminées par M. BROUGHAM, peuvent (abstraction faite de certaines mesures précises) s'énoncer ainsi.

* M. BROUGHAM nomme, fort à propos, *flexion*, ce que, jusqu'à lui, la plupart des physiciens nommoient inflexion, (et quelques autres diffraction.) Et il distingue la flexion en *inflexion* et *déflexion*: la première approchant le rayon, et la seconde l'éloignant, du corps flecteur.

1°. Le rayon le plus inflexible est aussi le plus déflexible.

2°. Le rayon le plus réfrangible est le moins flexible. Ainsi, le rayon rouge est à la fois plus infléchi, et plus défléchi, que le violet, dans les mêmes circonstances.

§. 16. La loi de la déflexion résulte bien des principes, dans ce qui concerne ce phénomène seul. Les rayons rouges démontrent ici, comme dans la réflexion, leur plus grande force de répulsion.

§. 17. Quant à la loi d'inflexion, elle ne résulte pas des principes qui expliquent la réflexion.

On pourroit dire, que les rayons les plus répulsifs sont aussi les plus attractifs. Cette proposition est admise par M. Brougham; mais la diverse réfrangibilité des rayons donne une indication toute contraire.

Soit pour la déflexion, soit pour l'inflexion, on est placé dans des circonstances pareilles à celles de l'expérience de réflexion par un très petit cylindre, (§. 5.) que j'ai discutée. (§. 10.) Les corps flecteurs sont comparables à ce petit cylindre. Ainsi l'angle d'incidence n'est pas donné immédiatement, et on a lieu de croire qu'il est égal à l'angle de déflexion : il doit de même être égal à l'angle d'inflexion ; mais il doit arriver que l'observateur ne s'apperçoive pas de cette égalité.

Du reste, le phénomène de l'inflexion est plus compliqué, parceque la courbe décrite par le rayon doit avoir deux rebroussemens.

On peut entrevoir quelque explication de ses lois, qui au premier coup d'oeil paroissent choquer des principes reconnus : mais je m'abstiens de toute tentative pareille.

§. 18. Seconde Question. Les principes reconnus pour la réflexion, se concilient-ils avec ceux de la réfraction ?

Oui. Dès que les rayons ont traversé la sphère répulsive, ils entrent dans l'attractive : les rayons rouges sont, à la vérité, les plus répulsifs, mais rien n'empêche que les violets ne soient plus attractifs. On peut même dire, que ces deux faits se lient naturellement, et qu'on a quelque lieu de s'attendre que les rayons moins faciles à repousser, seront plus faciles à attirer.

Or, la réfraction les montre tels. Car, 1°. rien de mieux prouvé en optique theorique, que la proposition qui établit, que la réfraction est produite par une attraction dirigée perpendiculairement à la surface réfringente. (NEWTON, *Princip. l.* 1. *prop.* 94.) 2°. Donc aussi, les différences de réfraction, et en particulier la plus grande réfrangibilité des rayons violets, sont produites par la même cause : conséquence confirmée par leur réflexibilité supérieure dans le milieu le plus dense.

§. 19. TROISIEME QUESTION. Les principes de la réfraction, peuvent-ils expliquer ceux de la flexion ?

Non. Ce qui reste inexpliqué, c'est la loi d'inflexion. Dans cette loi, les rayons rouges semblent plus attractifs, tandis que dans la réfraction c'est le contraire. S'il est quelque explication qu'on entrevoie, nous y avons renoncé. (§. 17.) Ainsi, cette partie des phénomènes de flexion reste encore pour nous comme isolée.

§. 20. " Les rayons de la lumière, ne sont-ils pas ré-
" fléchis, réfractés, et infléchis, par une seule et meme force,
" qui se déploie diversement en diverses circonstances ?" * Telle est la question que se faisoit NEWTON, au commencement du siécle, et qui ne me paroît pas résolue vers sa fin.

Il est vrai, qu'en énonçant les conséquences de ses recherches, M. BROUGHAM conclut ainsi : " the rays of light are reflected,

* *Opt. Qu.* 4

"refracted, inflected, and deflected, by one and the same power, "variously exerted in different circumstances."

Cela est, sans doute, fort probable, mais non encore démontré.

§. 21. La seule analogie nouvelle (et, sans doute, très-importante,) qu'a saisie M. Brougham, entre ces trois classes de phénomènes, est celle qui résulte des rapports harmoniques entre les parties distinctes des spectres colorés, produits par la réfraction, la réflexion, et la flexion.

§. 22. Le spectre par réflexion, peut-il être calculé exactement, d'après les principes posés ci-dessus, (§. 10.) afin de comparer le résultat de ce calcul à celui de l'expérience? Sa division harmonique est un rapport saisi entre ce phénomène et celui de la réfraction, qui fortifie l'opinion, déja si probable, sur l'identité du principe dont ces deux phénomènes dépendent. Je n'ose aller au delà.

§. 23. Sans doute, en pesant ces considérations, on se rappellera la proposition avancée par M. Brougham, savoir, que "la réflexibilité des rayons est comme leur réfrangibilité in-"versement." Mais il faut bien remarquer le sens de cette assertion.

Newton a fait sortir un rayon du verre à l'air, par la face plane d'un prisme, sous un angle d'incidence connu; et, ayant observé l'angle de réfraction des rayons rouges, et des violets, il a trouvé ces angles, et leurs sinus, comme il suit*

Angle commun d'incidence	31°	15′	0″	sinus 50
Angle de réfraction du rouge	53	4	58	—— 77
Angle de réfraction du violet	54	5	2	—— 78

M. Brougham a fait tomber un rayon sur la convexité d'une

* *Opt. l. 1. part. 1. prop. 7.*

fibre métallique, sous un angle d'incidence inconnu; et, ayant observé la réflexion, il en a conclu les angles et sinus suivans.

Angle d'incidence commun	-	77° 20′	sinus	$77\frac{1}{2}$
Angle de réflexion du rouge	-	75 50	——	77
Angle de réflexion du violet	-	78 51	——	78

On voit bien, en effet, que les nombres 77 et 78 expriment, de part et d'autre, des limites, d'un côté de réfraction, de l'autre de réflexion; mais on n'en peut pas conclure la raison inverse des quantités qu'on mesure dans l'observation de ces deux phénomènes: la disparité des circonstances de ces deux expériences s'y oppose. M. Brougham fait remarquer lui-même la différence d'incidence. Mais ce n'est pas la seule; et il suffit de se rappeller, que par une même incidence la dispersion des rayons colorés varie selon la nature des milieux, pour détruire toute idée de proportion régulière, exprimée d'une manière précise et générale, sans égard à la diversité des milieux.*

La proposition affirmée par M. Brougham, veut donc dire seulement, que celui des rayons qui occupe le plus de place sur le spectre réfracté, en occupe moins sur le spectre réfléchi; et que l'un et l'autre offre la division harmonique. Cela suffit bien pour indiquer quelque analogie, mais non pour fonder, sans aucune autre preuve, l'unité du principe.

§. 24. C'est dans le même sens qu'il faut entendre la proposition qui établit que "les flexibilités sont comme les réflexibilités "directement, et comme les réfrangibilités inversement." Et il y a encore beaucoup plus de disparité dans les circonstances,

* Voyez entr'autres les conclusions que M. Robert Blair a tiré de ses expériences, aussi exactes que multipliées, sur ce sujet. *Trans. of the R. S. of Edinburgh, Vol. III. p. 72.*

et dans les résultats, comme M. Brougham a soin de le faire remarquer. Ainsi, la division harmonique du spectre coloré fournit une analogie encore beaucoup plus foible, en faveur de l'identité d'un principe commun, auquel ces trois phénomènes doivent être rapportés.

Et cependant il faut convenir que ces foibles analogies entraînent la persuasion ; et que l'esprit ne sera point satisfait, jusqu'à ce que, par quelque nouvel effort, la flexion soit réunie aux phénomènes de réflexion et de réfraction, par l'unité de principe.

§. 25. Notre ignorance sur la nature des forces qui produisent ces phénomènes, en particulier sur la nature de la force répulsive, et le défaut d'accord qui règne encore entre les phénomènes de flexion et les autres, permettent-ils d'avoir confiance à la cause physique, indiquée dès long-temps par Newton, reprise récemment, et même calculée, par M. Brougham ; je veux dire, la différence de masse des particules qui composent les divers rayons ?

§. 26. Est-on en droit d'envisager comme une hypothèse, la théorie Newtonienne des accès de facile et difficile transmission ? Cette théorie n'est que l'expression généralisée d'un fait bien observé. Si les transmissions et réflexions alternatives ne dépendent que de l'épaisseur des lames transparentes, il faut que les rayons, ou le milieu, soient alternativement, et en tempuscules égaux, dans des dispositions opposées. Des expériences plus variées feront voir si l'épaisseur seule y influe : c'est sous ce point de vue que l'Abbé Mazeas avoit entrepris quelques expériences, qui ne donnoient encore aucun résultat, mais qu'on pouvoit espérer de voir suivre avec succès, (*Mem. des Sav. Etr.* 1755.) La théorie des lames minces transparentes, paroît avoir été conçue par Newton dès l'âge de 27 ans, et il ne l'a publié que

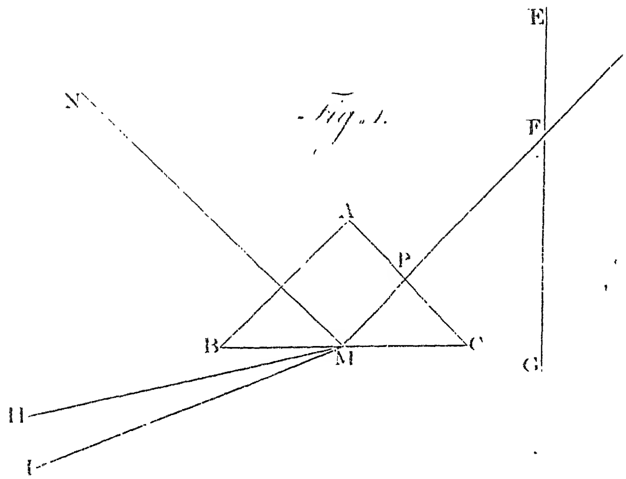


Fig. 1.

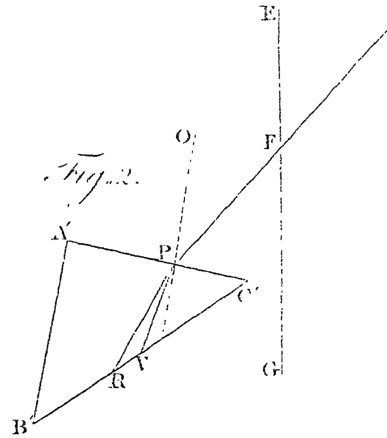


Fig. 2.

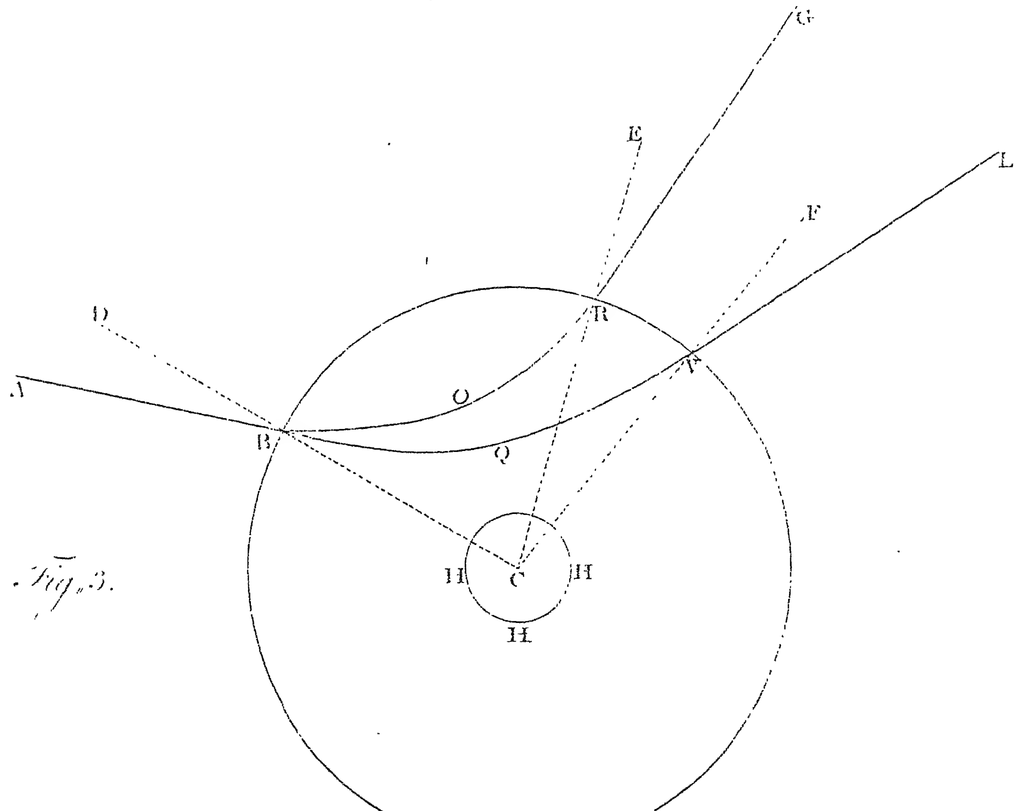


Fig. 3.

35 ans plus tard : car il y faisoit allusion dans ses *Leçons*, à Cambridge, en 1669, et la 1ere édition de son *Optique* est de l'année 1704. (*Opusc. Tom. II. p.* 275.) Autant il seroit absurde de mettre en parallèle avec la raison l'autorité même la plus respectable, autant il est juste d'exiger un examen très attentif d'une opinion aussi réfléchie.

§. 27. Je finirai par observer, que l'explication que j'ai proposé, selon les principes NEWTONIENS, du phénomène observé par M. BROUGHAM, dans la réflexion opérée par un cylindre très petit, (§. 10.) ne nuit pas à l'emploi que ce physicien en fait, pour expliquer les couleurs des corps naturels. Son sentiment et celui de NEWTON, à cet égard, ne sont pas en contradiction. Il n'est pas sûr que les couleurs des corps naturels ne soient produites que d'une façon ; mais, pour qu'elles soient produites, il faut que la réflexion s'opère par chaque particule des corps, sous toute sorte d'angles. Et je ne vois pas que M. BROUGHAM ait réussi à opérer, sous plusieurs angles variés, la réflexion qu'il a obtenue par ses petits cylindres. Il semble qu'il ne parle, d'une manière précise, que de celle où l'angle d'incidence étoit d'environ 77°, et, par conséquent, fort grand.

XII. *An Account of the Orifice in the Retina of the human Eye, discovered by Professor* Soemmering. *To which are added, Proofs of this Appearance being extended to the Eyes of other Animals. By* Everard Home, *Esq. F. R. S.*

Read April 19, 1798.

HAVING had the honour of laying before this learned Society, at different times, observations on the structure of the eye, both in man and in other animals, I have been naturally led to avail myself of every opportunity to investigate this subject.

My attention has been recently directed to the prosecution of this inquiry, by a very curious discovery of an aperture in the retina of the human eye, which we owe to Mr. SOEMMERING, an anatomist of considerable reputation, resident at Mentz. His account of this discovery has been published on the Continent, but I do not know that any copy of the memoir has been brought into this country.

It was believed by Mr. SOEMMERING, and also by the French anatomists, that this appearance is only to be met with in the human eye. I have, however, been so fortunate as to discover it in the eyes of other animals; and the object of the present paper is, to lay before the Society the observations I have made upon this subject.

I am indebted to my friend Sir CHARLES BLAGDEN, for the first intelligence of Mr. SOEMMERING's discovery. I afterwards received a more particular account, in a very obliging letter

from Mr. MAUNOIR, an eminent surgeon at Geneva, which contains, I believe, the material information published by Mr. SOEMMERING: I shall therefore transcribe that part of the letter, which is as follows.

"The war being an obstacle to a free communication be-"tween England and the Continent, you are not, perhaps, ac-"quainted with a new discovery in the anatomy of the human "eye, made by a professor of Mentz, Mr. SOEMMERING; permit "me, therefore, to say something on the subject. He was dis-"secting, in the bottom of a vessel filled with a transparent "liquid, the eyes of a young man who had been drowned, and "was struck on seeing, near the insertion of the optic nerve on "the retina, a yellow round spot, and a small hole in the mid-"dle, through which he could see the dark *choroides*, (looking "at the surface of the retina which covers the vitreous hu-"mour.) He dissected other human eyes, and constantly, "when the dissection was carefully made, found the hole of "the retina seemingly at the posterior end of the visual ra-"dius, nearly two lines on the temporal side of the optic nerve, "and the hole surrounded by the yellow zone, of above three "lines in diameter. The hole of the retina is not directly seen, "being covered with a fold of the retina itself. An anatomist "of Paris dissected many eyes of quadrupeds and birds, and "found the yellow spot and hole in *no* animal but the human "kind.

"Should you think that nature has intended this hole to "grow large when the eye is opposed to a strong light, and "thereby cause a great part of the rays to fall on the choroid, "and *vice versa*, when the eye is in darkness? And the want of "such a construction in animals, is it owing to a greater power

"of augmenting or diminishing the pupil, than in men? If "Messrs. MARIOTTE and LE CAT should come to life again, "they would find, in that hole, the explanation of the phæno-"menon of the two cards, one disappearing at a certain dis-"tance from one eye, *&c.* which may be explained by saying, "that where the optic nerve enters the ball, there is no cho-"roid, and so no vision.

"I dissected some human eyes a short time after I had read "the discovery, and found the spot, the *ruga* concealing it, and "the yellow zone. The best way, I think, to see them, is to take "off the half posterior part of the sclerotica, then the corre-"spondent part of the choroid; both must be cut round the "insertion of the optic nerve. The retina is to remain bare and "untouched, sustaining alone the vitreous humour; then you "may see the round spot, which reaches the optic nerve, and a "fold of the retina, marking a diameter of the spot. Then, if "you press the ball a little with your finger, so as to push the "vitreous humour rather near the bottom of the eye, the *ruga* "is unfolded, and you will see the hole perfectly round, of $\frac{1}{4}$ of "a line in diameter, and its edges very thin.

"All this can be seen on the inside of the eye, but not so "perfectly; and, in that case, you must make your observa-"tions in water."

Many months elapsed, after the receipt of this letter, before I could procure an eye in a proper state for observing this aperture in the retina; but, in the course of last month, several opportunities offered, and I saw the appearance described by Mr. MAUNOIR very distinctly.

The mode I adopted for examining the retina, was that of removing the transparent cornea; then taking away the iris,

and wounding the capsule of the crystalline lens, so as to disengage the lens, without removing that part of the capsule which adheres to the vitreous humour; by which means, the retina remained undisturbed, and could be accurately examined, when a strong light was thrown into the eye.

The aperture in the retina, surrounded by a zone with a radiated appearance, was distinctly seen, on the temporal side of the insertion of the optic nerve, and about $\frac{1}{6}$ of an inch distant from it, apparently a little below the posterior end of the visual radius. The aperture itself, in this view, was very small. After having viewed it in two different eyes, I took an opportunity of showing it to Sir JOSEPH BANKS and Sir CHARLES BLAGDEN, who both saw it with the same degree of distinctness.

At first, I believed it necessary to have a very fresh eye for demonstrating this aperture, but I have since found, that it is more readily seen in an eye two days after death; the zone, which is the most conspicuous part, being of a lighter colour the first day, than it is upon the second.

I have also succeeded in preserving the posterior part of the eye in spirits, without destroying the appearance of this aperture. This preparation I am unwilling to bring to a public meeting of the Society, since it may be liable to be injured by being much shaken; but I hope my having shown it to Sir JOSEPH BANKS and Sir CHARLES BLAGDEN, will be sufficient evidence, both to the Society and others, that such a preparation can be made.

I am induced to make this remark, by recollecting that a celebrated anatomist of Edinburgh denied, in his last publication, that the anterior lamina of the cornea can be separated from the others, as a continuation of the tendons of the four

straight muscles of the eye, for no other reason than because he could not succeed in the demonstration of it; the failure, probably, arising from the eye not being sufficiently fresh to admit of such a separation. Had it been mentioned in my former paper, that the preparation, from which the engraving was made, had been shown to this learned Society, or to any members of it, my assertion would probably have had more weight.

In separating the vitreous humour from the retina, I found a greater adhesion at this particular part; and, when the vitreous humour was removed, the retina was pulled forward, forming a small fold, in the centre of which was this aperture. This doubling was sometimes produced by endeavouring to cut through the vitreous humour, to disengage the crystalline and its capsule.

I have been the more particular in describing the appearance of this aperture in the retina of the human eye, that, while I announce this curious discovery of Mr. SOEMMERING to this learned Society, I may give the most complete confirmation of it. To have this in my power affords me a particular pleasure, as it gives me an opportunity of doing justice to the merit of a foreign anatomist, who deserves so highly of our art; and who has demonstrated to his cotemporaries, that those who labour patiently, and follow their pursuits with ardour, may still hope to make discoveries, in the anatomy even of those parts of the body which are considered as the best understood; since the human eye, so long the favourite object of the most eminent anatomists and philosophers, is still but imperfectly investigated.

After having made the preceding observations upon this singular appearance in the human eye, I found, in Dr. DUNCAN'S

Annals of Medicine for 1797, an account of a publication concerning it by Professor REIL, entitled, The plait, the yellow spot, and the transparent portion of the retina of the eye.

After these are described separately, the following circumstances are mentioned. "SOEMMERING takes this appearance "to be a real hole. BUZZI, on the contrary, thinks that it is "merely a transparent and thin portion of the retina. "MICHAELIS seems to agree with him. REIL and MECKEL are "rather in favour of the existence of an actual hole.

"MICHAELIS saw the plait more distinctly in fœtuses of "seven or eight months, than in adults; and the transparent "portion lay concealed within it, but the yellow spot was "wanting: nor is it to be observed in the eyes of newly-born "children. After the first year, it becomes somewhat yellow, "and the depth of the colour increases with the age of the "subject. SOEMMERING says that this spot is pale in children, "bright yellow in young people, and becomes again pale in "old age. Its degree of saturation seems to be intimately con-"nected with the state of vision: it constantly diminishes, in "proportion as vision is obstructed. Where one eye only is "diseased, in it the yellow spot is wanting, and the plait is "small and wrinkled; while, in the sound one, they are rather "more distinct than usual.

"MICHAELIS discovered no vestige of these appearances in "the eyes of dogs, swine, or calves."

Professor REIL'S mode of dissecting the eye, to show the aperture and plait, is exactly similar to that mentioned in Mr. MAUNOIR'S letter.

It will appear, from the account of this orifice in the retina, which precedes these observations of Professor REIL, that the

plait so particularly mentioned is an artificial appearance, which takes place in the dissection of the eye, and arises from the circumstance of the vitreous humour adhering more firmly to the edge of this orifice, than to any other part of the retina; so that the smallest motion of the vitreous humour, in consequence of dividing it, or removing the choroid coat, produces a plait, by pulling forwards this portion of the retina. What is said of the colour of the yellow spot, and of the difference of opinion, whether it is a hole or a transparent portion of the retina, I shall consider more fully in another part of this Paper.

After having ascertained the appearance of this aperture in the human eye, and found what appeared the best mode of seeing it, I determined to investigate this subject in the eyes of other animals.

The monkey was the first animal which I procured for observation; being led, from previous knowledge in comparative anatomy, to believe that the structure of its eye must bear a very close resemblance to that of the human subject.

The eye was examined immediately after the death of the animal, and was prepared in the same way that I have already described the human eye to have been for this purpose; so that the concave surface of the retina appeared in its most natural state, and the vitreous humour, being entire, kept it expanded, and free from *rugæ*. On the first view, nothing was to be seen but one dark surface, surrounding the entrance of the optic nerve. Two hours after death, the retina became sufficiently opaque to be distinguished, and, immediately after, the orifice was visible, appearing to be an extremely small circular aperture, without any margin; but, in half an hour more, the zone had formed, which, when very accurately examined in a bright

light, had an appearance of four rays, at right angles, as expressed in the annexed plate. (See Tab. XVII. fig. 3.) Its situation, respecting the optic nerve, was precisely the same as in the human eye. As I considered this to be a fact of some importance, since it proved the aperture in the retina to be a part of the structure of the eye, generally, and not a peculiarity in the human eye, I requested Sir JOSEPH BANKS, Sir CHARLES BLAGDEN, and Dr. BAILLIE, to examine it: to all of them it appeared very distinct. After having shown it to those gentlemen, and having an accurate drawing made of it, I preserved that portion of the eye in spirits; where the aperture in the retina can still be distinctly seen, but the radiated appearance is lost.

In the eye of a bullock, prepared in the same manner, I looked in vain for a similar appearance: if it existed, and bore any proportion to the size of the eye-ball, as it appears to do in the human eye and that of the monkey, it must have been very visible. The concave surface of the retina was examined in different lights, under a variety of circumstances, and by magnifying glasses of different powers, but still no aperture could be discovered. I was, however, very much struck, while looking at the optic nerve, to see something in the vitreous humour, (in consequence of a person accidentally shaking the table,) that had not been before observed.

This proved to be a semi-transparent tube, resembling in its coats a lymphatic vessel, rising from the retina, close to the optic nerve, on the temporal side of its insertion, and coming directly forwards into the vitreous humour, in which it was lost, after being distinctly seen for $\frac{4}{20}$ths of an inch of its course.

Its appearance is accurately delineated in the annexed plate. (Fig. 4.)

This tube is not so distinctly seen in the eye immediately upon the animal's death, as some hours after; and is much more obvious in some eyes than in others. As the coats of the tube must be nearly the same in all eyes, this difference probably arises from its contents not always having the same degree of transparency.

When the eye has been kept 24 hours after the animal's death, there is an appearance of a zone of a circular form, a shade darker than the rest of the eye, in which the optic nerve is included: when this zone, which is nearly $\frac{7}{20}$ths of an inch in diameter, is attentively examined, the tube I have described is exactly in the centre of it. The tube seems to be confined by the vitreous humour, (while that humour is entire,) and only to move along with the central part of it; and, in some instances, when the vitreous humour is divided, the tube falls down. Its attachment at the retina appears stronger than its lateral connection with the vitreous humour; for, when I coagulated the vitreous humour in spirits, and separated it from the retina, I found the tube was left with the retina, but upon being touched was easily torn.

In the sheep's eye there is a similar tube, in exactly the same situation, respecting the optic nerve, but much shorter, and much less easily detected. It does not appear to be more than $\frac{1}{20}$th of an inch in length, before it is lost in the vitreous humour. After having seen the tube distinctly in two different eyes, and having had a drawing made of it, I looked for it in several others, without finding it: but, examining an eye from which the crystalline lens had not been removed, only an aperture

made into the vitreous humour, by removing a portion of the ciliary processes along with the iris, the tube was distinctly seen. The weight of the lens probably pulled forward the vitreous humour, and kept the short tube erect, in its natural situation.

I mention this circumstance, to prevent, as much as I am able, other anatomists from being disappointed in not finding it; which may readily happen, if the search be not made with considerable attention.

In the sheep, there is no appearance of a zone surrounding the tube.

These facts, although few in number, are sufficient to prove, that this orifice is not peculiar to the retina of the human eye; and that its situation in man and in the monkey is the same: in them, it is placed at some distance from the optic nerve; but, in some other animals, its situation is close to that nerve, and it puts on the appearance of a tube, instead of an orifice.

There is one circumstance which is curious, and which it will require further information upon this subject to explain; the yellow zone, found in the human eye and that of the monkey, is not met with in any other animal which I have examined.

Having stated the facts, and also the opinions of other anatomists, that have come to my knowledge, as well as my own observations, upon this orifice in the retina of the human eye, discovered by Mr. Soemmering, and having added to these, several new facts respecting it in other animals, I shall draw some general conclusions from the whole, with a view to show that the conjectures which have been made, respecting its use, are probably erroneous. I shall afterwards point out several

reasons for considering it as the orifice of a lymphatic vessel intended to carry off the vitiated parts of the vitreous humour and crystalline lens.

In the human subject, as no examination can be made for some considerable time after death, it is impossible to ascertain what is the real state of this orifice in the living eye, and what changes take place in it after death; we only learn, that the tinge of yellow surrounding the orifice is very slight, when the eye is examined recently, and that the next day it becomes much deeper.

These points appear to be satisfactorily cleared up, by the examination that was made of the monkey's eye, as it was begun before the parts had lost the appearance belonging to them as living parts. In that state, the retina was transparent, and no orifice could be seen; so that the orifice is rendered visible, by remaining transparent, while the surrounding retina becomes opaque. This appears to decide the dispute between Messrs. SOEMMERING and BUZZI; for, if this part does not undergo the change peculiar to the retina, we must consider the retina as wanting there. After the orifice is thus rendered visible, the yellow tinge is wanting, and does not take place for several hours, and even then is fainter than it becomes afterwards; which appears to be sufficient evidence, that this tinge is the effect of some change after death, and cannot, therefore, have any effect upon vision.

The orifice has been supposed to account for a small object becoming invisible, when placed at a certain distance from the eye, and brought opposite a particular part of the retina. This, however, cannot be the case, as its situation in the retina does not correspond with the part opposed to the object, when rendered invisible.

The orifice itself is probably too small to produce any defect in vision, as the trunks of the blood-vessels which ramify upon the retina cover a larger space than this orifice, for a considerable extent, without obstructing the sight of any part of the object.

While my observations were confined to the human eye, I was led to consider this orifice as a lymphatic vessel, passing from the vitreous humour through the retina, but could bring no absolute proof of its being so. This opinion was strengthened by finding, that in the monkey, the orifice was only rendered visible when the retina became opaque; and it has since been corroborated, by a distinct tube being met with in the eyes of sheep and bullocks.

That a change must be constantly taking place in the crystalline and vitreous humours, to preserve to them the necessary degree of transparency, can hardly be doubted; and that the absorbent vessels which perform that office should have one common trunk, which follows the course of the artery and vein, perfectly agrees with what takes place in other parts of the body.

In the human eye, and that of the monkey, the artery is in the centre of the optic nerve; but that would have been too circuitous a course for the lymphatic vessel to follow, and, by going through the retina, at some distance from the nerve, it can pass out of the orbit with the blood-vessels that go through the *foramen lacerum orbitale inferius.* In the bullock and sheep, there is a plexus of vessels surrounding the optic nerve, and the tube dips down, close by the optic nerve, probably to accompany them.

From the observations made by Michaelis, of the yellow spot not being visible in fœtuses, or in infants under a year old,

or in eyes that are blind, also of its being brighter in young people, and paler in old, it would appear, that it is only when the eye is capable of performing its functions, that there is any stain communicated to the retina.

EXPLANATION OF THE PLATE (Tab. XVII.)

The drawings from which the figures are engraved were made from preparations of the eye lying in water, with a strong light shining upon the preparation. In making the drawings, the principal object was, procuring a distinct view of the parts surrounding the optic nerve; when this could be obtained, the situation of the eye itself was not attended to.

Fig. 1. A transverse section of the human eye, immediately before the ciliary processes. The retina is viewed through the posterior portion of the capsule of the crystalline lens.

a. The termination of the optic nerve.

b. The aperture in the retina, discovered by Professor Soemmering.

Fig. 2. A longitudinal section of the left eye in the human subject, to show the relative situation of the aperture in the retina to the entrance of the optic nerve, and the mode in which it appears to project, when the vitreous humour is disturbed.

a. The termination of the optic nerve.

b. The aperture in the retina.

Fig. 3. A transverse section of the eye of a large monkey, to show the aperture in the retina: its situation is the same as in the human eye. The zone has the appearance of a star with four rays.

a. The entrance of the optic nerve.

b. The aperture in the retina.

Fig. 1.

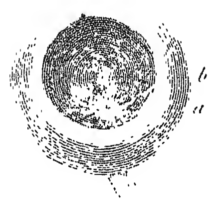


Fig. 2.

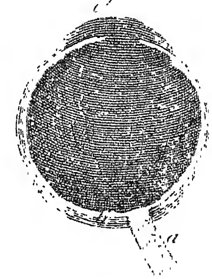


Fig. 3.

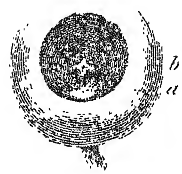


Fig. 5.

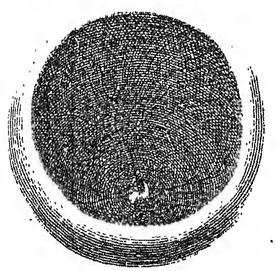


Fig. 4.

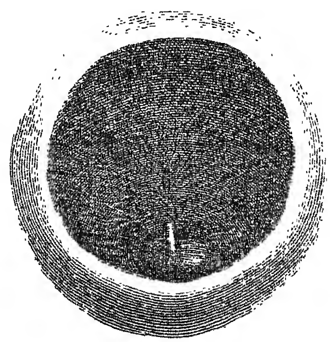


Fig. 4. A transverse section of the eye of a bullock, to show that there is a semi-transparent tube projecting from the edge of the entrance of the optic nerve, into the vitreous humour. This tube is surrounded by a zone, with a distinct margin: it is situated on the temporal side of the optic nerve.

Fig. 5. A transverse section of the eye of a sheep, to show that there is a similar tube as in the bullock, in the same situation, but much shorter, and without the surrounding zone.

XIII. *A Description of a very unusual Formation of the human Heart. By Mr.* James Wilson, *Surgeon. Communicated by* Matthew Baillie, *M. D. F. R. S.*

Read May 3, 1798.

THE heart is an organ of so much importance in the animal œconomy, and is so immediately concerned in the support of life, that any unusual deviation from its natural form and situation in the human body, has always been considered as a subject of some interest by the physiologist; such deviations have, therefore, not unfrequently been submitted to the consideration of this and other learned Societies. Many circumstances respecting the circulation of the blood, and respiration, wholly unknown to our ancestors, have lately been ascertained; but we are not as yet arrived at a perfect knowledge of these important actions. Difficulties yet remain; more information may still be acquired; and the reasoning upon these subjects will be less liable to fallacy, in proportion to the number of facts which have been observed, and the accuracy of the observations. These are the reasons which have induced me to lay before this Society, a description of a monstrosity in the human heart, very singular in its nature, and which, I believe, has not hitherto been observed or described. I have consulted the works of those authors who were the most likely to have

recorded such cases, but I have not been able to meet with a description of any which have been at all similar.

It is well known to most of the Members of this Society, that the circulation of the blood throughout the body, and exposure of it to the atmospheric air in respiration, seem, in most animals, to be necessarily connected; but are not equally so in all. They are so much connected in the human subject, and in most quadrupeds, that after birth there is a double heart; *viz.* one for the circulation of the blood throughout the body, to be subservient to the various purposes of life and growth; the other for its circulation through the lungs, where it undergoes a change which is essential to its general circulation through the body: these two circulations, in the natural state, bear an exact proportion to each other. Instances, however, have occurred, even in the human subject, where this exact proportion has not been preserved; yet life has been prolonged for some years, but in a feeble and imperfect state. In some of these instances, the pulmonary artery has been smaller than usual, so that much less than the natural quantity of blood was exposed to the influence of the air in the lungs; in others, the foramen ovale has not been closed, but a considerable communication has remained between the two auricles; and, in others, there has been a communication between the two ventricles, from a deficiency in the septum. The effect of all these deviations is the same, upon the blood in the general circulation, *viz.* that a part of the blood is not exposed to the air in the lungs; so that it is less pure as it circulates over the body. A more remarkable deviation in the structure of the heart, than any to which I have just alluded, has been lately published by Dr. Baillie, in his Morbid Anatomy. In this heart, the aorta

arose from the right ventricle, and the pulmonary artery from the left; the reverse of what ought, in the regular course of circulation, to have taken place; (the veins were as usual;) and no communication was found between the one vessel and the other, except through the remains of the ductus arteriosus, which was not larger than a crow quill, and a small part of the foramen ovale, which still continued open; yet this child lived for two months. In the following case of monstrous formation of the heart, there is this very great singularity, that nature seems to have substituted, very exactly, the circulation which takes place in some amphibious animals, for that which is natural to the human species.

The infant had arrived at its full time, and lived seven days after its birth. Instead of the usual integuments, muscles, *&c.* a membranous bag appeared to protrude on the upper and fore part of the abdomen, extending from the last bone of the sternum some way below the middle of the belly, and outwards, so as to be nearly circular: the navel-string seemed to enter this membrane near its middle, and to wind superficially, for some little way, towards the left side; it then dipped into the abdomen, at the place where this membrane joined the usual coverings. Within this bag, the appearance of which was very nearly similar to that of the chorion and amnios which envelop the fœtus at birth, but thicker in consistence, a tumour was perceived, possessing considerable motion, from the nature of which, no doubt was entertained that it was the heart.

During the short period of the child's life, it was seen and examined by a number of professional men. Upon its death, the tumour was carefully opened by Mr. MORELL, in the pre-

sence of Dr. Poignand; when the heart, as was previously suspected, appeared to be situated in the epigastric region of the abdomen, and to be imbedded, as it were, in a cavity formed on the superior surface of the liver. In this state, the child was sent to Dr. Baillie, by whose desire I injected the heart, and laid its principal vessels bare, so as to bring their uncommon distribution and course into view: a preparation of them still remains in Dr. Baillie's possession.

A considerable part of the tendinous portion of the diaphragm appeared to be wanting, as likewise the lower part of the pericardium, which is usually affixed to it. The thorax being laid open on each side of the sternum, the two pleuræ were seen passing from that bone to the spine, and covering the lungs, as usual. The lungs appeared perfectly natural in colour, and nearly so in shape; but were larger and fuller than usual, in consequence of more room being afforded for them in the thorax, from the peculiar situation of the heart. In the space corresponding to the anterior mediastinum, was the thymus gland, considerably longer than in other children, and extending downwards the whole length of the sternum; behind this, was a peculiar arrangement of blood-vessels.

The heart, instead of consisting of four cavities, as in the natural structure, consisted of a single auricle and ventricle, which were each of them large in their size. A large arterial trunk arose from the ventricle, and ascended into the thorax, between the pleuræ, immediately behind the thymus gland: it soon divided into two large branches, one of which continued to ascend, forming the aorta; the other passed backwards, and proved, upon examination, to be the pulmonary artery.

The aorta, having reached the common place of its curva-

ture, formed it in the same manner as it usually does; sent off the vessels belonging to the head and upper extremities; descended before the vertebræ, and passed into the abdomen between the crura of the diaphragm. From the place where it began to form the arch, it was in no respect different from the aorta of any other infant, except that no bronchial artery was sent to the lungs, from it or any of its ramifications.

The vessel which proved to be the pulmonary artery, almost immediately divided into two branches; one going to the lungs of the left, the other to the lungs of the right side. Upon measuring accurately the circumference of the aorta, where it separated from the original trunk, it was found to be exactly one inch and a quarter. Upon measuring the circumference of the pulmonary artery, in the same manner, it was found to be fifteen sixteenths of an inch; so that it was five sixteenths of an inch less than the aorta.

The vena cava inferior, having been partly surrounded by the substance of the liver, entered the lower and back part of the auricle. The subclavian vein of the right side crossed over to the left of the mediastinum, where it joined the left subclavian, and formed the vena cava superior. This passed down on the left of the ascending, and before the descending, part of the aorta; it was then joined by a trunk formed by two large veins, which came out of the lungs, and which were situated immediately behind the pulmonary arteries: the union of this trunk with the vena cava superior was continued into a large vessel, which gradually expanded itself into the auricle. The vena azygos ascended on the left side; received some branches which passed under the aorta from the right, and then entered the upper and back part of the vena cava superior: there were

no bronchial veins. From there being neither bronchial arteries nor veins, it would appear that the pulmonary arteries and veins, in addition to their usual offices, performed those of the bronchial vessels.

The liver was not divided on its upper surface by the suspensory ligament, but had a considerable cavity scooped, as it were, out of its substance; which, in shape, was adapted to, and contained, the heart: it was also, in some other particulars, rather different from its natural shape, but not sufficiently so to require being minutely described. The rest of the infant was examined, but was not found to be dissimilar to any other. These circumstances are expressed by the accompanying figures of the parts when dissected; (Tab. XVIII.) in taking of which, much attention was paid to render them very accurate.

It is a well ascertained fact, that the blood receives a florid hue from the influence of the air on it in the lungs; and this change is supposed to be effected by the combination of a certain quantity of oxygen gas with it. In passing from the arteries to the veins, in every part of the body except the lungs, it loses the florid hue, and becomes darker: the florid blood is that which is employed for the purposes of supporting life. In the natural circulation, it is well known, that the whole of the blood conveyed to, and circulating in, the pulmonary artery, is of a dark colour; and the whole of it, when returned by the pulmonary veins, is florid.

It is obvious, in the case which I have described, that there always must have been florid and dark-coloured blood mixed, and circulating in the arteries. It would seem also, upon the first reflection, that the quantity of dark-coloured blood would be the greatest, in the same proportion as the capacity of the

aorta was larger than that of the pulmonary artery. It is therefore necessary to recollect, that a considerable proportion of the blood carried to the lungs was already florid or oxygenated; and also, that the lungs in this infant were larger in proportion, than in children of the same age: a smaller quantity of blood, therefore, was to be oxygenated, and a larger surface than usual was appropriated for this purpose. It appears also, from experiments, (such as making a person breathe air in which there is a greater proportion of oxygen gas than in our atmosphere,) that the blood can combine with more of it than it does in natural respiration; it therefore is not an improbable supposition, that a larger quantity was combined here. A small drawback must be allowed, for the quantity of oxygenated blood used in the support and secretions of the lungs, and which is usually conveyed to them by the bronchial artery; but this quantity is too small to require more than this slight observation of it. The blood also which passed to the lungs, must have been again conveyed to the heart sooner, from the shortness of its circuit; and must have entered the heart with a quicker or stronger current, than that blood which passed to, and was returned from, the more remote parts of the body; as, in this child, the pulmonary artery and aorta were filled by the contraction of the same ventricle. In the hearts of other children, some time after birth, the muscular fibres of the right side are much fewer in number than in the left.

If these circumstances are admitted as fact, *viz.* that the blood circulating through the lungs of this child was combined with a larger proportion of oxygen gas, and was returned in a quicker and stronger current into the auricle than that returned by the venæ cavæ, it seems reasonable to infer, that this

blood, mixing and blending with the dark or unoxygenated blood, would render the whole nearly as much oxygenated as it usually is found in the left side of the heart, and in the aorta; therefore, that the blood circulating in the arteries of this child would be fully equal to the support of life. Previous to birth, this peculiarity of structure could not affect its health or growth, as the placenta then answers the purpose which the lungs do afterwards; and the single ventricle seemed as equal, from its size, to propel the blood on to the placenta, as both ventricles in the natural state are, by means of their communication through the ductus arteriosus.*

The inference which has been drawn seems further confirmed, from the colour and heat of this child, during life, being not perceptibly different from those of other children. In all those cases of malformation of the heart where the foramen ovale, or the ductus arteriosus, has continued open; or where the septum of the ventricles has been perforated, and the pulmonary artery small, (and at the same time two ventricles,) it has been observed, that the body had a livid colour, and, in general, that there was a deficiency of heat.

From the particular inquiries which I made, concerning the heat and colour of this child, of the professional gentlemen who saw it during life, and of the nurse who attended and

* It is here not unworthy of remark, that the circulation in this child, after its birth, was in several circumstances similar to the circulation in other children previous to that period. A child, before birth, may be said to have a single heart; as both the auricles communicate together, by means of the foramen ovale; and the pulmonary artery communicates with the aorta, by means of the ductus arteriosus. Hence, in the fœtal heart, the blood returned from the body, which is of a dark colour, and the blood returned from the placenta, which is florid, are poured into the same auricle; the blood which is sent to the placenta is therefore already in part oxygenated.

dressed it, I found that the heat, so far as could be judged by the feeling, (for it was not tried by the thermometer,) was in no respect different from that of other children; and that the colour of the skin was perfectly natural, except that, on the day on which it was born, and a short period before its death, the lips occasionally had something of a livid appearance; but that this did not last any time, as they were generally pale. This occasional lividness would happen to a child in that state, should the heart and circulation be in no way different from what they naturally are.

I could meet with no other remarkable circumstances, either in the history of the mother during pregnancy, or in the child after birth. It cried occasionally, like other children, but seemed weak, and in pain; it slept; it sucked heartily, even a few hours before its death, and had apparently healthy evacuations of urine and fæces.

Its death can be satisfactorily accounted for, from another cause than the extraordinary formation of its heart and blood-vessels. The membranous covering, on the fore part of the abdomen, did not appear to possess sufficient vascularity to retain its life after birth; for it immediately lost its living principle, and became putrid and mouldy in parts. Previous to the child's death, a process of separation had begun, between it and the living parts to which it was connected, and a line of inflammation was distinctly seen. Had this process been completed, and the slough thrown off, the heart would have been exposed; but, before this, the heart itself had inflamed; which was proved from its being found covered with a coat of coagulable lymph recently thrown out, and from this inflammation its death must have arisen.

Had the heart been covered with the usual parietes of the abdomen, it is probable, notwithstanding its situation, that this child might have lived in a tolerable state of health for years; but must constantly have been exposed to have its heart injured by some external accident, from its not being defended by the ribs and the sternum.

The formation and disposition of the heart and vessels, in this child, resemble much those which are found in the frog, and some other amphibious animals; but this infant could not, like them, be amphibious. Those animals are extremely tenacious of life, so that they live some time, even after their heart and lungs are removed from their bodies; and, as their circulation can go on without respiration, it is therefore not wonderful that they often live a considerable time without change of air. Life, in the human species, depends equally on both these actions; for death takes place, if either of them should stop. The circulation of the blood in this infant would have met with no impediment, had it been immersed in water; but, unless respiration went on, which in that state it could not do, the blood could undergo no change in the lungs; and this change is equally essential to the support of life, as the circulation of the blood.

EXPLANATION OF THE FIGURES. (Tab. XVIII.)

Fig. 1. represents the heart, blood-vessels, liver, &c. as they appeared when dissected; part of the ribs, the sternum, thymus gland, lungs, &c. having been removed.

AA. The heart, consisting of one auricle and one ventricle.

B. A large arterial trunk, arising from the ventricle.

C. The aorta, arising from this trunk.

D. The pulmonary artery, arising from the same trunk.

E. The vena cava superior, descending on the left side.

F. F. The pulmonary veins, entering into the auricle with the vena cava superior.

G. The vena azygos, ascending on the left side.

H. The diaphragm, adhering laterally to the margin of the chest, but deficient on the fore part, towards the sternum.

I I. The liver.

K. The cavity on the upper surface of the liver, in which the heart was in part situated.

L. The membranous covering turned downwards.

M. The umbilical vein.

Fig. 2. represents the heart. The aorta and pulmonary artery are cut off near their origin, to shew the pulmonary veins, and vena cava superior, entering the auricle.

A. The auricle.

B. The ventricle.

C. The trunk from which the aorta and pulmonary artery arose.

D. A large vessel entering the auricle, and receiving the blood from the pulmonary veins and vena cava superior.

E. The trunk formed by the pulmonary veins.

F. The vena cava superior.

G. The vena azygos.

H. The right subclavian vein.

I. The left ditto.

Fig. 1.

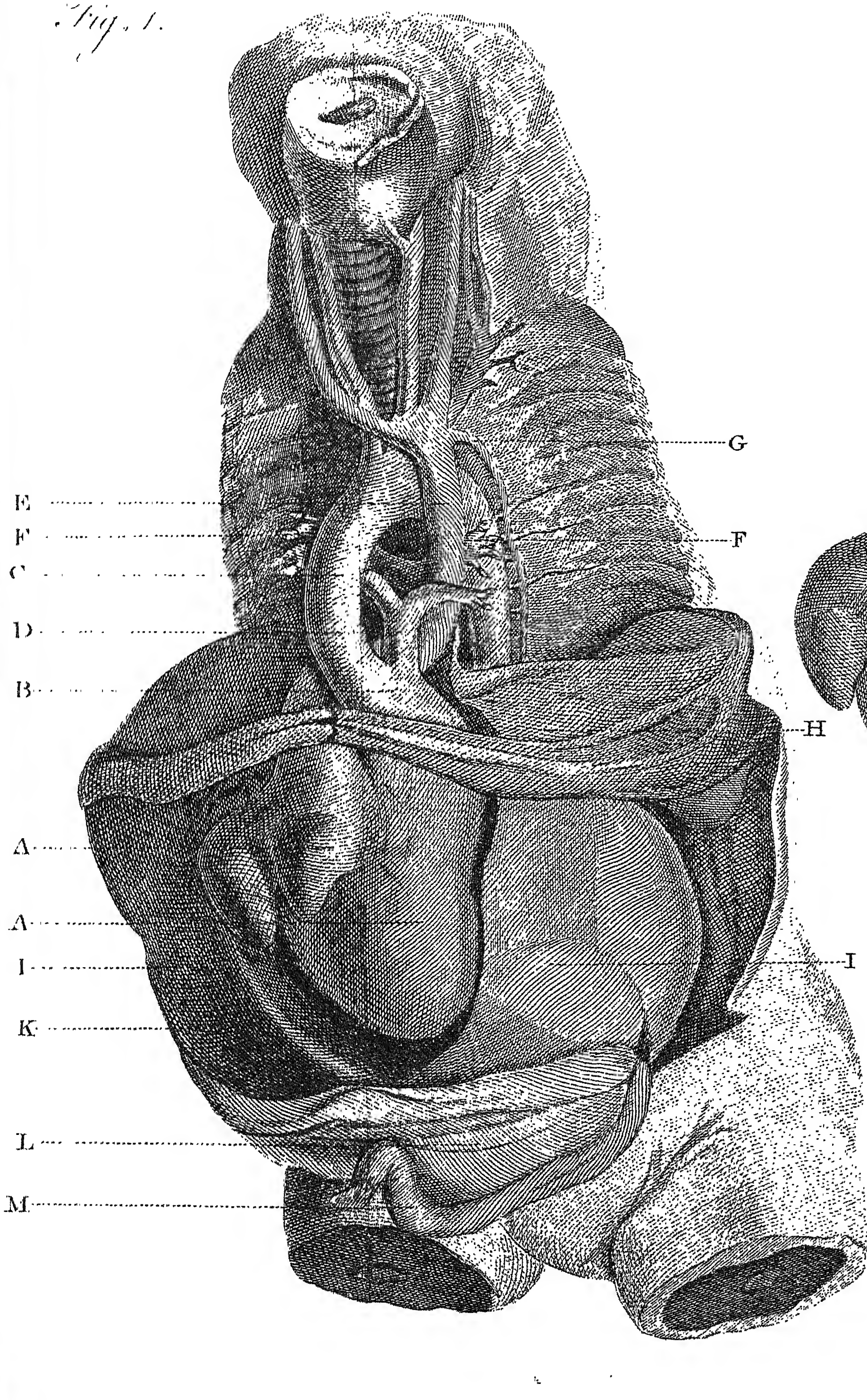
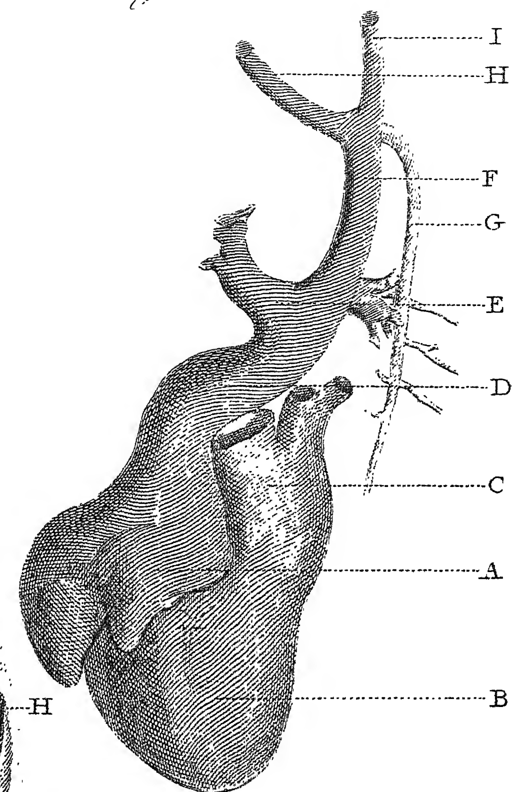


Fig. 2.



XIV. *Account of a singular Instance of atmospherical Refraction. In a Letter from* William Latham, *Esq. F.R.S. and A.S. to the Rev.* Henry Whitfeld, *D.D. F.R.S. and A.S.*

Read May 10, 1798.

DEAR SIR, *Hastings, August 1, 1797.*

On Wednesday last, July 26, about five o'clock in the afternoon, whilst I was sitting in my dining-room at this place, which is situated upon the Parade, close to the sea shore, nearly fronting the south, my attention was excited by a great number of people running down to the sea side. Upon inquiring the reason, I was informed that the coast of France was plainly to be distinguished with the naked eye. I immediately went down to the shore, and was surprised to find that, even without the assistance of a telescope, I could very plainly see the cliffs on the opposite coast; which, at the nearest part, are between forty and fifty miles distant, and are not to be discerned, from that low situation, by the aid of the best glasses. They appeared to be only a few miles off, and seemed to extend for some leagues along the coast. I pursued my walk along the shore to the eastward, close to the water's edge, conversing with the sailors and fishermen upon the subject. They, at first, could not be persuaded of the reality of the appearance; but they soon became so thoroughly convinced, by the cliffs gradually appearing more elevated, and approaching nearer, as it

were, that they pointed out, and named to me, the different places they had been accustomed to visit; such as, the Bay, the Old Head or Man, the Windmill, *&c.* at Boulogne; St. Vallery, and other places on the coast of Picardy; which they afterwards confirmed, when they viewed them through their telescopes. Their observations were, that the places appeared as near as if they were sailing, at a small distance, into the harbours.

Having indulged my curiosity upon the shore for near an hour, during which the cliffs appeared to be at some times more bright and near, at others more faint and at a greater distance, but never out of sight, I went upon the eastern cliff or hill, which is of a very considerable height, when a most beautiful scene presented itself to my view; for I could at once see Dengeness, Dover cliffs, and the French coast, all along from Calais, Boulogne, *&c.* to St. Vallery; and, as some of the fishermen affirmed, as far to the westward even as Dieppe. By the telescope, the French fishing-boats were plainly to be seen at anchor; and the different colours of the land upon the heights, together with the buildings, were perfectly discernible. This curious phænomenon continued in the highest splendour till past eight o'clock, (although a black cloud totally obscured the face of the sun for some time,) when it gradually vanished.

Now, Sir, as I was assured, from every inquiry I could possibly make, that so remarkable an instance of atmospherical refraction had never been witnessed by the oldest inhabitant of Hastings, nor by any of the numerous visitors, (it happening to be the day of the great annual fair, called Rock-fair, which always attracts multitudes from the neighbouring places,)

I thought an account of it, however trifling, would be gratifying to you.

I should observe, the day was extremely hot, as you will perceive by the subjoined rough journal of a small thermometer, which was kept in the dining-room above mentioned. I had no barometer with me, but suppose the mercury must have been high, as that and the three preceding days were remarkably fine and clear. To the best of my recollection, it was high water at Hastings about two o'clock P. M. Not a breath of wind was stirring the whole of the day; but the small pennons at the mast-heads of the fishing-boats in the harbour, were, in the morning, at all points of the compass.

I am, &c.

WILLIAM LATHAM.

P.S. I forgot to mention that I was, a few days afterwards, at Winchelsea, and at several places along the coast; where I was informed, the above phænomenon had been equally visible. I should also have observed, that when I was upon the eastern hill, the cape of land called Dengeness, which extends nearly two miles into the sea, and is about sixteen miles distant from Hastings, in a right line, appeared as if quite close to it; as did the fishing-boats, and other vessels, which were sailing between the two places: they were likewise magnified to a great degree.

State of the Thermometer at Hastings, during the Month of July, 1797.

1797.	Therm.	Time.	Wind.	Weather.
July 1	64	10 A. M.	SW	Windy. Fair.
2	64	10	SW	Windy. Fair.
3	62	10	SW	Rain. Windy.
4	62	10	SW	Fair. Windy.
5	61	10	SW	Rain. Windy.
6	60	10	SW	Rain. Windy.
7	61	10	W	Rain. Windy.
8	62	10	NW	Fine.
	66	5 P. M.	NW	Fine.
9	66	10 A. M.	SW	Fine.
10	67	10	N afterw. SW	Fine.
11	65	10	SW	Foggy all day.
12	63	10	SW	Fine.
13	72	10	SW	Fine.
14	76	10	W	Fine.
	68	12	W	Fine.
15	72	10	W	Fine.
16	72	10	N	Fine.
	78	7 P. M.	E	Storm of wind. Lightning.
17	73	10 A. M.	W	Fine.
18	70	10	W	Fine. Showers in the night.
19	67	10	WSW	Fine. Windy.
20	67	10	SW	Rain. Windy.
21	65	10	SW	Fine. Windy.
22	61	10	S	Rain.
23	65	10	S	Fine.
24	66	10	S	Fine.
25	66	10	SW	Fine.
26	68	10	SW	Fine. Dead calm all day.
	76	5 P. M.	SW	Fine.
27	72	10 A. M.	SW	Fine.
28	70	10	S	Fine.
29	72	10	E	Fine.
30	70	10	SW	Rain.
31	69	10	S	Fine. Windy.

XV. *Account of a Tumour found in the Substance of the human Placenta. By* John Clarke, *M. D. Communicated by the Right Hon. Sir* Joseph Banks, Bart. *K. B. P. R. S.*

Read May 17, 1798.

WHILST the structure and uses of any part of the animal body remain unknown, every new fact or occurrence ought to be recorded; since, by this means only a more perfect knowledge of it can be expected to be obtained.

As there are few subjects more interesting than those which concern the functions of animals, and more especially those processes by which they are originally formed, and afterwards sustained, I beg leave to submit the following Paper to the attention of the Royal Society, supposing it not to be foreign to the general views of the institution.

The exertions of the most patient industry have been hitherto baffled, in the attempt to detect the first changes which succeed that process by which animals are propagated.

If the object of immediate pursuit has not been obtained, much light, in the course of the investigation, has been collaterally thrown upon the growth and nutrition of animals, both in the egg state of oviparous animals, and in the uterine state of such as are viviparous.

The structure of the egg of oviparous animals serves to elucidate the corresponding process in the viviparous; and, although in many cases analogies are very inconclusive, yet in this the

resemblance is so close, that the latter may be said to be demonstrated by the former.

A certain *temperature*, *nourishment*, and the application of *vital air*, (or oxygen,) seem to be essential to the evolution of the young of oviparous animals.

As the young are expelled from the mother, contained in the cavity of the egg, at a very early period of their existence, and as afterwards they have no connection whatsoever with her, these are supplied by various contrivances; and the mode of application has been very distinctly explained, by modern inquirers into the structure of eggs.

Since then the same substances are to be produced, and supported, in viviparous as in oviparous animals, the conclusion is reasonable, that similar means should be employed to attain similar ends.

It is easy to conceive how *warmth* may be imparted to a fœtus situated in the uterus.

The materials for *nourishment*, it receives from the placenta: but the precise manner in which they are supplied has not yet been discovered. Of the fact there can be no doubt, because there are many cases on record, in which there could be no other possible way by which support could be had.*

With respect to *vital air*, (or oxygen,) the young of all viviparous animals, whilst in the uterus, live in the same medium as fishes, and have a structure similar to gills, for the exposure of their blood to it: this structure is the placenta.

The heart of the fœtus is adapted to this mode of life, and in effect consists but of one auricle and one ventricle, as it is

* A case of this kind I described some time ago, which is published in the Philosophical Transactions for the year 1793.

found to do in fishes. The junction between the two ventricles is attended with a great advantage, in performing the circulation through the placenta; where the length and convolution of the umbilical vessels, in some animals, offer a great resistance to the force of the heart, and render more exertion necessary.

In the superior aorta, the circulation is carried on by the left ventricle alone; as the ductus arteriosus does not join the aorta, till after the latter has given off the carotid and subclavian branches.

Vital air is communicated to the blood of the embryo, as it is to the blood of fishes. This, in its passage through the gills, is exposed to water, which is allowed by all to contain a large proportion of vital or oxygen gas, and returns thence fitted to answer the purposes of life.

In like manner, the blood of the mother, in the cells of the placenta, having received the essential part of this gas from her lungs, is applied to the capillary vessels of the umbilical arteries, which receive and transmit it to the embryo; the life of which so entirely depends upon this communication, that an obstruction to the circulation through the placenta, for the space of two or three minutes, will sometimes irrecoverably destroy it.

The gills of fishes form a permanent part of their bodies; because they are designed to pass the whole of their lives in the same medium. This is not the case in the embryo of viviparous animals; which, after birth, is to change its situation for another, in which there is a direct exposure of the blood to atmospheric air. For this reason, the placenta, whose use is only temporary, is attached to the fœtus by a slender connection, which is soon dissolved after birth.

I have thought it necessary to introduce the foregoing obser-

vations upon the structure and functions of the placenta, in order to shew that the principal use of it is to transmit, and apply respectively to each other, the blood of the fœtus, and that of its mother. No other action is carried on by the vessels of the fœtal portion of the placenta, as far as is yet known, than what has been described, unless so much as may be necessary for their own growth and nourishment.

The tumour which gave occasion to this Paper is, however, an instance to prove, that these vessels are capable, like those in other parts, of forming solid organized matter; and that very considerable deviations from the ordinary structure of the placenta may exist, and be perfectly compatible with the life and health of the fœtus.

Previously to the birth of a healthy child, an amazing quantity of liquor amnii was evacuated, which was by accident received in a vessel, and, being afterwards measured, was found to amount to two gallons, Winchester measure.

When the placenta came away, a hard solid body was found in its substance. It was preserved by Mr. MAINWARING, under whose care the case occurred, and was by him obligingly presented to me.

Fine injection was thrown into the arteries and vein of the funis umbilicalis: when they were filled, they appeared to be enlarged thrice beyond their natural size.

The placenta, thus prepared, was subjected to examination. Its anterior surface was found to be covered with the amnion, behind which lay the chorion, as usual. Some branches, both of the arteries and veins, coming from the funis, ramified in the common manner, forming the fœtal portion of the placenta. Others, of a very large size, not less than a swan's quill, were

sent to the tumour; which was situated behind the chorion, and lay imbedded in the fœtal portion of the placenta. The general form of this tumour was oval, about four inches and a half long, and three inches broad. The thickness of it was about three inches. It weighed upwards of seven ounces.

Its shape resembled that of a human kidney; one edge being nearly uniformly convex, whilst the other, where the vessels approached it, was a little hollowed.

The general character of the surface of the tumour was convexity; but in some parts of it there were slight indentations, more particularly in the course of the large vessels.

The whole of the tumour was inclosed in a firm capsule, in the substance of which the large vessels were contained, nearly in the same manner as they are found in the dura mater. In the interstices of the vessels, the capsule did not appear to be vascular; at least there were no vessels capable of carrying the injected matter.

The blood-vessels, branching off from the funis to supply the tumour, partly went over one side, and partly over the other side of the tumour; ramifying as they ran, till, meeting at the convex edge of the tumour, they anastomosed very freely. From the large trunks on the surface, small branches were given off, penetrating into the substance, and supplying the whole tumour with blood.

Upon making a section through the tumour, in the direction of its length, the consistence was found to be uniform, firm, and fleshy, very much resembling, in this respect, the kidney. The cut surface, upon examination, had somewhat of a mottled appearance; some parts being highly vascular, whilst others were white and uninjected.

If the mere existence of such a tumour is not to be considered as a disease, there was no appearance of any morbid tendency in any part of it. The whole structure seemed to consist of a regularly organized matter throughout, supplied with vessels exclusively belonging to itself, and not passing to it from the surrounding parts, as is generally the case in diseased masses.

They who are inclined to consider every new appearance in the structure of parts as disease, may be disposed to include this under that appellation.

But disease consists of such an alteration in the structure, or functions, of a part, as occasions the natural operations of it to be imperfectly performed, or entirely arrested. This tumour appears to have produced no such effect: all the common and known functions of the placenta were performed, notwithstanding the existence of this substance: the child had been as well nourished, and the benefits arising from the application of vital air or oxygen, to its blood, just as well supplied, as if the tumour had not existed.

It cannot be said of this, as it might of some tumours, that it would in time have shewn marks of a morbid tendency, so as to have deranged the common actions of the placenta; because, when gestation terminates, the life, and all the uses of the placenta, are at an end.

I am disposed, therefore, to consider this fleshy substance, as a solitary instance of a formative property in the vessels of the placenta; which they have not been hitherto generally known to possess.*

* The placenta sometimes becomes converted into a mass of hydatids, connected to each other by small filaments; but this must be considered as a disease, inasmuch as the natural structure is destroyed, and it directly interferes with the offices of the pla-

There was a remarkable circumstance attending this case, which ought not to be lost sight of, *viz.* the extraordinary quantity of liquor amnii, which had been contained in the ovum. What connection there was between this and the tumour, cannot be absolutely explained from a single instance, as there did not seem to be any direct communication between the tumour and the cavity of the amnion. The whole of it lay, as has been before related, behind the chorion; so that, between it and the cavity of the ovum, there were two membranes interposed. In its organization, it had all the appearance of a glandular part, and was extremely vascular; but, upon a very attentive examination of it, no duct could be found leading from it into the cavity of the ovum.

Yet, although it may appear difficult to prove, that the quantity of liquor amnii depended upon this substance, still, as it so considerably exceeded that which is found in common, or has ever been described, it is reasonably to be believed that it did so.

The manner, however, by which the secreted fluid was conveyed from the tumour into the general cavity of the ovum, must still remain unaccounted for.

centa, which no longer performs perfectly the functions for which it was designed. Nourishment and vital air are no longer supplied properly to the fœtus, which therefore commonly dies.

EXPLANATION OF THE PLATES.

(Tab. XIX.)

A view of the fœtal surface of the placenta, with the arteries and vein injected.

a. The funis umbilicalis; the vein is exhibited to shew its increased size.

b b b. The shaggy vessels of the chorion, forming the fœtal portion of the placenta.

c. The cavity, or cyst, in which the tumour (vide Tab. XX.) lay.

(Tab. XX.)

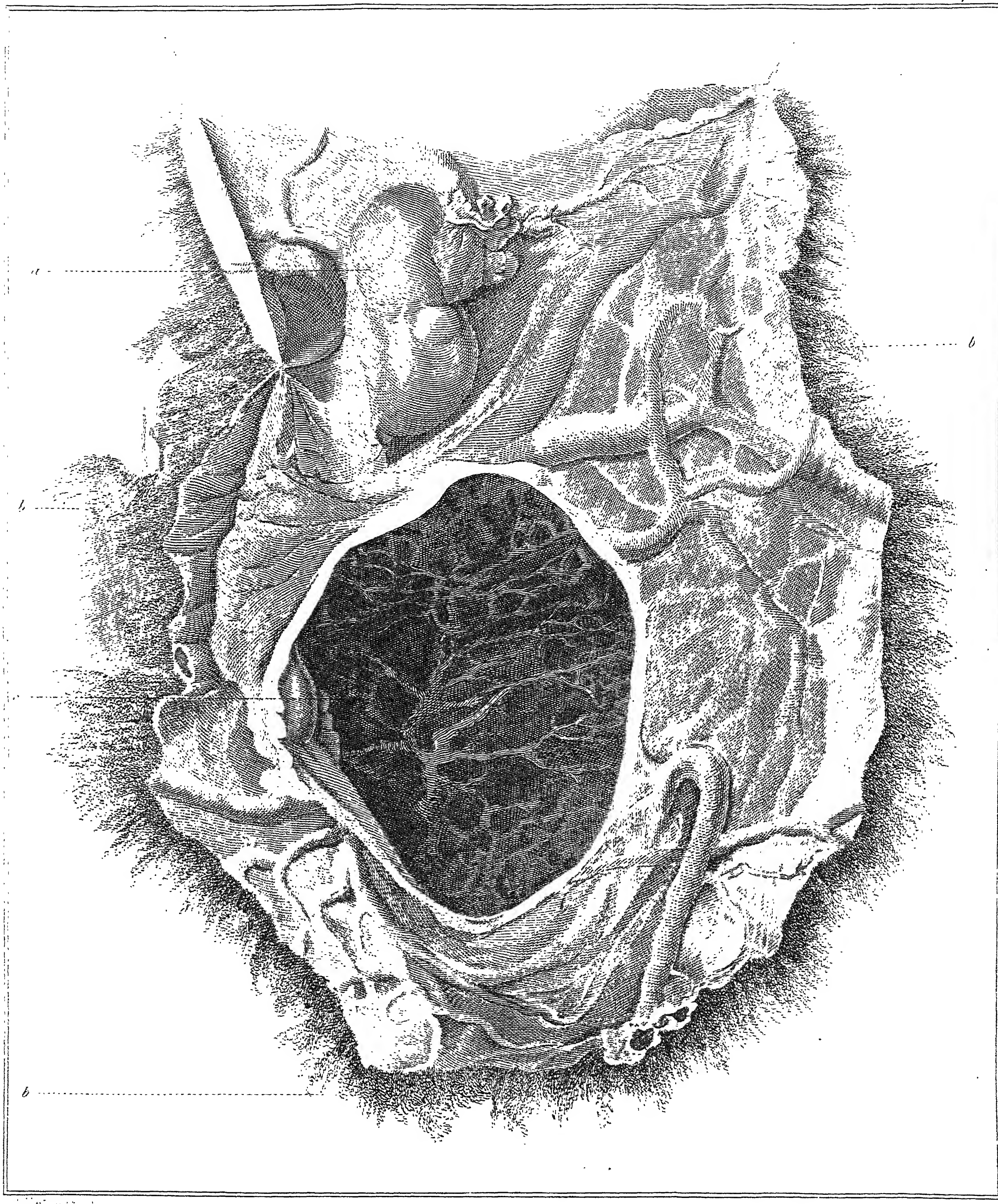
An external view of the tumour, contained in the cyst formed in the substance of the placenta, as seen in Tab. XIX.

a. A branch of the umbilical artery entering the tumour.

b. The vein returning the blood from the tumour to the umbilical vein.

c. A square portion of the capsule which contained the tumour, turned down to shew the internal structure.

d. The substance of the tumour, seen through the opening left by dissecting off the capsule.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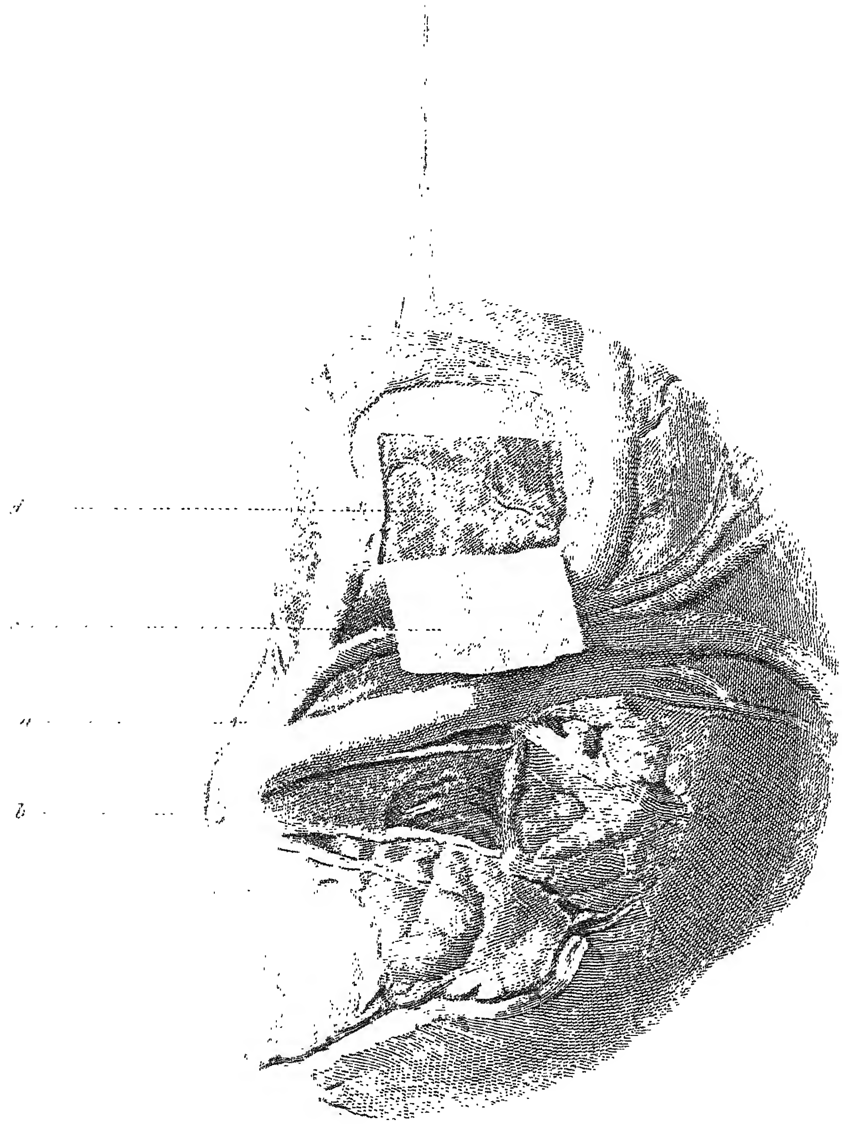
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XVI. *On the Roots of Equations. By* James Wood, *B.D. Fellow of St. John's College, Cambridge. Communicated by the Rev.* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read May 17, 1798.

THE great improvements in algebra, which modern writers have made, are chiefly to be ascribed to VIETA's discovery, that "every equation may have as many roots as it has dimensions." This principle was at first considered as extending only to positive roots; and even when it was found that the number might, in some cases, be made up by negative values of the unknown quantity, these were rejected as useless. It could not, however, long escape the penetration of the early writers on this subject, that in many equations, neither positive nor negative values could be discovered, which, when substituted for the unknown quantity, would cause the whole to vanish, or answer the condition of the question. In such cases, the roots were said to be impossible, without much attention to their nature, or inquiry whether they admit of any algebraical representation or not. As far as the actual solution of equations was carried, *viz.* in cubics and biquadratics, the imaginary roots were found to be of this form, $a + \sqrt{-b^2}$; and subsequent writers, from this imperfect induction, concluded in general, that every equation has as many roots, of the form $a \pm \sqrt{\pm b^2}$, as it has dimensions. In the present state of the science, this proposition is

of considerable importance, and its truth ought to be established on surer grounds. The various transformations of equations, the dimensions to which they rise in their reduction, and the circumstances which attend their actual solution, are most easily explained, and most clearly understood, by the help of this principle. Mr. EULER appears to have been the first writer who undertook to give a general proof of the proposition; but, whatever may be thought of his reasoning in other respects, as he carries it no further than to an equation of four dimensions, and it does not appear capable of being easily applied in other cases, it gives us no insight into the subject. Dr. WARING's observations upon the proposition are extremely concise;* and, to common readers, it will still be a matter of doubt, whether a quantity of any description whatever will, when substituted for x in the expression $x^8 - px^7 + qx^6 - \ldots + w$, cause the whole to vanish.

In the investigation of the proof here offered, it became necessary to attend to the method of finding the common measure of two algebraical expressions; and to observe particularly, in what manner new values of the indeterminate quantities are introduced; and how they may again be rejected. It appears, that these values are necessary in the division; and, when they have been thus introduced, they enter every term of the *second* remainder, from which they may be discarded. This circumstance enables us, not only to determine the nature of the roots of every equation, but also affords us a direct and easy method of reducing any number of equations to one, and obtaining the final equation in its lowest terms.

* *Meditationes Alg.* p. 272.

PROP. I.

To find a common measure of the quantities $ax^n + bx^{n-1} + cx^{n-2} + dx^{n-3} + \&c.$ and $Ax^{n-1} + Bx^{n-2} + Cx^{n-3} + Dx^{n-4} + \&c.$

In order to avoid fractions, multiply every term of the dividend by A^2, the square of the coefficient of the first term of the divisor, and the operation will be as follows:

$$\left.\begin{matrix}Ax^{n-1} + Bx^{n-2} \\ + Cx^{n-3} + \&c.\end{matrix}\right) aA^2x^n + bA^2x^{n-1} + cA^2x^{n-2} + dA^2x^{n-3} + \&c. \left(aAx + bA - aB\right.$$

$$aA^2x^n + aBAx^{n-1} + aCAx^{n-2} + aDAx^{n-3} + \&c.$$

$$* \; (bA^2 - aBA)x^{n-1} + (cA^2 - aCA)x^{n-2} + (dA^2 - aDA)x^{n-3} + \&c.$$

$$(bA^2 - aBA)x^{n-1} + (bBA - aB^2)x^{n-2} + (bCA - aBC)x^{n-3} + \&c.$$

$$(P) \quad * \quad (cA^2 - bBA + aB^2 - aCA)\,x^{n-2} + (dA^2 - bCA + aBC - aDA)\,x^{n-3} + \&c.$$

Let $(cA - bB)\,A + (B^2 - CA)\,a = \alpha$

$(dA - bC)\,A + (BC - DA)\,a = \beta$

$(eA - bD)\,A + (BD - EA)\,a = \gamma$ &c.

and the first remainder (P) is $\alpha x^{n-2} + \beta x^{n-3} + \gamma x^{n-4} + \&c.$ proceed with this as a new divisor, and the next remainder (Q) will be $(\overline{C\alpha - B\beta}\,.\,\alpha + \overline{\beta^2 - \alpha\gamma}\,.\,A)\;x^{n-3} + (\overline{D\alpha - B\gamma}\,.\,\alpha + \overline{\beta\gamma - \alpha\delta}\,.\,A)\;x^{n-4} + \&c.$

Respecting this operation we may observe:

1. That were not every term of the first dividend multiplied by A^2, that quantity would be introduced by reducing the terms of the remainder (P) to a common denominator.

2. When $P = o$, $Ax^{n-1} + Bx^{n-2} + Cx^{n-3} + \&c.$ is a divisor of $A^2\,(ax^n + bx^{n-1} + cx^{n-2} + \&c.)$; and therefore it is a divisor of $ax^n + bx^{n-1} + cx^{n-2} + \&c.$ unless it be a divisor

of A^2, which is impossible; consequently no alteration is, in this case, made in the conclusion, by the introduction of A^2.

3. When P does not vanish, then every divisor of P is a divisor of $Ax^{n-1} + Bx^{n-2} + Cx^{n-3} +$ &c. and of $A^2 (ax^n + bx^{n-1} + cx^{n-2} +$ &c.); and therefore of $ax^n + bx^{n-1} + cx^{n-2} +$ &c. unless $A^2 = o$, in which case the remainder, P, becomes $aB(Bx^{n-2} + Cx^{n-3} +$ &c.), every divisor of which is a divisor of $Bx^{n-2} + Cx^{n-3} +$ &c. whether it be a divisor of $ax^n + bx^{n-1} + cx^{n-2} +$ &c. or not. That is, there are two values of the indeterminate quantity A, which, if retained, will produce erroneous conclusions.

4. A^2 enters every term of the second remainder (Q), and the two values, before introduced, may therefore be again rejected.

The coefficient of the first term of this remainder is $\overline{C\alpha - B\beta} \,.\, \alpha + \overline{\beta^2 - \alpha\gamma} \,.\, A$; and, by substituting for α, β and γ, their values, and retaining only those terms in which A is not found, and those in which it is only of one dimension, we have

$$\begin{aligned}
C\alpha &= -bBCA + aCB^2 \\
&\; - aC^2A \\
-B\beta &= +bBCA - aCB^2 \\
&\; + aBDA \\
\hline
C\alpha - B\beta &= -aC^2A + aBDA \\
\overline{C\alpha - B\beta} \,.\, \alpha &= -a^2B^2C^2A + a^2B^3DA \\
\overline{\beta^2 - \alpha\gamma} \,.\, A &= a^2B^2C^2A - a^2B^3DA;
\end{aligned}$$

therefore, those parts of $\overline{C\alpha - B\beta} \,.\, \alpha + \overline{\beta^2 - \alpha\gamma} \,.\, A$, in which A is of one dimension, and in which it is not found, vanish.

In the same manner it appears, that A^2 enters every other term of the remainder Q.

5. If the remainder $Q = o$, then, by the second observation, the introduction of α^2 in the division, produces no error in the conclusion; and, if Q do not vanish, α^2 will be found in every term of the third remainder, and may there be rejected; and so on. Thus we obtain the conclusion, without any unnecessary values of A, B, C, &c. or a, b, c, &c.

6. If the highest indices of x, in the original quantities, be equal, it will only be necessary to multiply the terms of the dividend by A, which may be rejected after the second division. If the difference of the highest indices of x be m, the terms of the dividend must be multiplied by A^{m+1}, the first quotient being carried to $m + 1$ terms. This quantity, A^{m+1}, will enter every factor in each term of the second remainder.

7. If it be necessary to continue the division, let

$$\overline{C\alpha - B\beta} \,.\, \alpha + \overline{\beta^2 - \alpha\gamma} \,.\, A = m A^2$$
$$\overline{D\alpha - B\gamma} \,.\, \alpha + \overline{\beta\gamma - \alpha\delta} \,.\, A = n A^2$$
$$\overline{E\alpha - B\delta} \,.\, \alpha + \overline{\beta\delta - \alpha\varepsilon} \,.\, A = p A^2$$
&c.

and the third remainder is $(\overline{\gamma m - \beta n} \,.\, m + \overline{n^2 - mp} \,.\, \alpha)\, x^{n-4}$ $+ (\overline{\delta m - \beta p} \,.\, m + \overline{np - mq} \,.\, \alpha)\, x^{n-5} + (\overline{\varepsilon m - \beta q} \,.\, m + \overline{nq - mr} \,.\, \alpha)$ x^{n-6} + &c. every term of which is divisible by α^2. The law of continuation is manifest.

PROP. II.

Two roots of an equation of $2m$ dimensions may be found by the solution of an equation of $m \,.\, \overline{2m - 1}$ dimensions.

Let $x^{2m} + px^{2m-1} + qx^{2m-2} + rx^{2m-3} +$ &c. $= 0$; and, if possible, let v and z be so assumed that $v + z$, and $-v + z$, may be two roots of this equation; then,

$$\left.\begin{array}{lll} x^{2m} & = v^{2m} \pm 2mz\,v^{2m-1} + 2m.\frac{2m-1}{2}.z^2 v^{2m-2} \pm 2m.\frac{2m-1}{2}.\frac{2m-2}{3}.z^3 v^{2m-3} + \text{\&c.} \\ p\,x^{2m-1} & = \quad \pm\ p\,v^{2m-1} + \overline{2m-1}.\,pz\,v^{2m-2} \pm \overline{2m-1}.\frac{2m-2}{2}.\,pz^2 v^{2m-3} + \text{\&c.} \\ q\,x^{2m-2} & = \qquad\qquad q\,v^{2m-2} \pm \overline{2m-2}.\,qz\,v^{2m-3} + \text{\&c.} \\ r\,x^{2m-3} & = \qquad\qquad\qquad \pm\ r\,v^{2m-3} + \text{\&c.} \end{array}\right\} = 0$$

and consequently,

$$\left.\begin{array}{l} v^{2m} + 2m.\frac{2m-1}{2}.z^2 \\ +\ \overline{2m-1}.\,pz \\ +\ q \end{array}\right\} v^{2m-2} + \text{\&c.} + \left.\begin{array}{l} z^{2m} \\ +\ p\,z^{2m-1} \\ +\ q\,z^{2m-2} \\ +\ r\,z^{2m-3} \\ +\ \text{\&c.} \end{array}\right\} = 0$$

and also,

$$\left.\begin{array}{l} 2mz \\ +\ p \end{array}\right\} v^{2m-1} + \left.\begin{array}{l} 2m.\frac{2m-1}{2}.\frac{2m-2}{3}.z^3 \\ +\ \overline{2m-1}.\frac{2m-2}{2}.\,pz^2 \\ +\ \overline{2m-2}.\,qz \\ +\ r \end{array}\right\} v^{2m-3} + \text{\&c.} + \left.\begin{array}{l} 2m\,z^{2m-1} \\ +\ \overline{2m-1}.\,pz^{2m-2} \\ +\ \overline{2m-2}.\,qz^{2m-3} \\ +\ \overline{2m-3}.\,rz^{2m-4} \\ +\ \text{\&c.} \end{array}\right\} v = 0$$

Assume $y = v^2$; and let the coefficients of the terms of the former equation be 1, b, c, d, &c. and of the latter, A, B, C, D, &c. and the equations become

$$y^m + by^{m-1} + cy^{m-2} + dy^{m-3} + \text{\&c.} = 0$$
$$\mathrm{A}y^{m-1} + \mathrm{B}y^{m-2} + \mathrm{C}y^{m-3} + \text{\&c.} = 0.$$

These equations have a common measure of the form $y \pm \mathrm{Z}$, where Z is expressed in terms of z and known quantities; and this common measure may be found, by dividing, as in Prop. 1. till y is exterminated, and making the last remainder equal to nothing.

Now, the first remainder is $(\overline{c\mathrm{A} - b\mathrm{B}}.\mathrm{A} + \mathrm{B}^2 - \mathrm{CA})\,y^{m-2} + (\overline{d\mathrm{A} - b\mathrm{C}}.\mathrm{A} + \mathrm{BC} - \mathrm{DA})\,y^{m-3} + (\overline{e\mathrm{A} - b\mathrm{D}}.\mathrm{A} + \mathrm{BD} - \mathrm{EA})\,y^{m-4} + \text{\&c.}$; or, by substitution, $\alpha y^{m-2} + \beta y^{m-3} + \gamma y^{m-4} + \text{\&c.}$

and, in α, z rises to 6 dimensions; in β, to 8 dimensions; and, in γ, to 10 dimensions, &c.

The second remainder is $(\overline{C\alpha - B\beta}.\alpha + \overline{\beta^2 - \alpha\gamma}.A)\,y^{m-3} + (\overline{D\alpha - B\gamma}.\alpha + \overline{\beta\gamma - \alpha\delta}.A)\,y^{m-4} +$ &c.; or, by substitution, $mA^2y^{m-3} + nA^2y^{m-4} +$ &c. and, dividing by A^2, the dimensions of z in m, are 15; in n, 17, &c. Let π, $\varkappa$, ϱ, σ, τ, &c. be the dimensions of z in the first term of the 1st, 2d, 3d, 4th, 5th, &c. remainders; then

$$\begin{aligned}
\pi &= 6 \\
\varkappa &= 15 \\
\varrho &= 2\varkappa - \pi + 4 \\
\sigma &= 2\varrho - \varkappa + 4 \\
\tau &= 2\sigma - \varrho + 4 \\
&\text{\&c.}
\end{aligned}$$

the increment of the $m-1$ term of this series is $4m+1$, and therefore the $m-1$ term itself is $2m.\overline{m-1}+m$, or $m.\overline{2m-1}$. Now, in the $m-1$ remainder, y does not appear, and, in that remainder, z rises to $m.\overline{2m-1}$ dimensions; if then, this remainder be made equal to nothing, and a value of z determined, the last divisor, $y \mp Z$, where Z is some function of z, is known; and this is a common measure of the two equations $y^m + by^{m-1} + cy^{m-2} +$ &c. $= 0$, and $Ay^{m-1} + By^{m-2} + Cy^{m-3} +$ &c. $= 0$; consequently, $y \mp Z = 0$; and $y = \pm Z$; hence $\pm\sqrt{y}$, or $v, = \pm\sqrt{\pm Z}$; therefore, by the solution of an equation of $m.\overline{2m-1}$ dimensions, two roots, $z \pm \sqrt{\pm Z}$, of the original equation, are discovered.

Cor. 1. Since two roots of the proposed equation are $z+v$, and $z-v$, $x^2 - 2zx + z^2 - v^2 = 0$ is a quadratic factor of that equation.

Cor. 2. In the same manner that the two equations

$y^m + by^{m-1} + cy^{m-2} + \&c. = 0$, and $Ay^{m-1} + By^{m-2} + Cy^{m-3} + \&c. = 0$, are reduced to one, may any two equations be reduced to one, and one of the unknown quantities exterminated; also, the conclusion will be obtained in the lowest terms.

PROP. III.

Every equation has as many roots, of the form $a \pm \sqrt{\pm b^2}$, as it has dimensions.

Case 1. Every equation of an odd number of dimensions has, at least, one possible root; and it may, therefore, be depressed to an equation of an even number of dimensions.

Case 2. If the equation be of $2m$ dimensions, and m be an odd number, then $m.\overline{2m-1}$ is an odd number, and consequently z and v^2 (see Prop. II.) have possible values; therefore the proposed equation has a quadratic factor, $x^2 - 2zx + z^2 - v^2 = 0$, whose coefficients are possible; that is, it has two roots of the specified form; and it may be reduced two dimensions lower.

Case 3. If m be evenly odd, or $\frac{m}{2}$ an odd number, then the equation for determining z, has either two possible roots, or two of the form $a \pm b\sqrt{-1}$, (Case 2.); and v^2 will be of the form $c \pm d\sqrt{-1}$; hence, one value of the quadratic factor $x^2 - 2zx + z^2 - v^2 = 0$, will be of this form, $x^2 - \overline{2a + 2b\sqrt{-1}}.x + AB + CD\sqrt{-1} = 0$; and another of this form, $x^2 - \overline{2a - 2b\sqrt{-1}}.x + AB - CD\sqrt{-1} = 0$; consequently, $x^4 - 4ax^3 + (2AB + 4a^2 + 4b^2)x^2 - (4aAB + 4bCD)x + A^2B^2 + C^2D^2 = 0$, will be a factor of the proposed equation; and this biquadratic may be resolved into two quadratics, whose

coefficients are possible, and whose roots are therefore of the form specified in the proposition.

In the same manner the proposition may be proved, when $\frac{m}{4}$, $\frac{m}{8}$, $\frac{m}{16}$ &c. is an odd number; and thus it appears that it is true in all equations.*

Cor. 1. If v^2, or y, be positive, the roots of the quadratic factor $x^2 - 2zx + z^2 - v^2 = 0$, and therefore two roots of the proposed equation are possible. If $y = 0$, two roots are equal; and if y be negative, two roots are impossible.

Cor. 2. If a possible value of z be determined, and substituted in b, c, d, &c. the original equation will have as many pairs of possible roots as there are changes of signs in the equation $y^m + by^{m-1} + cy^{m-2} + \text{&c.} = 0$; and as many pairs of impossible roots as there are continuations of the same sign.

* See Dr. WARING's Med. Alg.

XVII. *General Theorems, chiefly Porisms, in the higher Geometry. By* Henry Brougham, *Jun. Esq. Communicated by Sir* Charles Blagden, *Knt. F. R. S.*

Read May 24, 1798.

The following are a few propositions that have occurred to me, in the course of a considerable degree of attention which I have happened to bestow upon that interesting, though difficult branch of speculative mathematics, the higher geometry. They are all in some degree connected; the greater part refer to the conic hyperbola, as related to a variety of other curves. Almost the whole are of that kind called porisms, whose nature and origin is *now* well known; and, if that mathematician to whom we owe the first distinct and popular account of this formerly mysterious, but most interesting subject,* should chance to peruse these pages, he will find in them additional proofs of the accuracy which characterizes his inquiry into the discovery of this singularly beautiful species of proposition.

Though each of the truths which I have here enunciated is of a very general and extensive nature, yet they are all discovered by the application of certain principles or properties still more general; and are thus only cases of propositions still more extensive. Into a detail of these, I cannot at present enter: they compose a system of general methods, by which the discovery of propositions is effected with certainty and ease; and

* See Mr. Playfair's Paper, in Vol. III. of the Edinburgh Trans.

they are, very probably, in the doctrine of curve lines, what the ancients appear to have prized so much in plain geometry; although, unfortunately, all that remains to us of that treasure, is the knowledge of its high value. Neither have I added the demonstrations, which are all purely geometrical, granting the methods of tangents and quadratures: I have given an example, in the abridged synthesis, of what I consider as one of the most intricate. It is unnecessary to apologize any farther for the conciseness of this tract. Let it be remembered, that were each proposition followed by its analysis and composition, and the corollaries, scholia, limitations, and problems, immediately suggested by it, without any trouble on the reader's part, the whole would form a large volume, in the style of the ancient geometers; containing the investigation of a series of connected truths, in one branch of the mathematics, all arising from varying the combinations of certain data enumerated in a general enunciation.*

As a collection of curious general truths, of a nature, so far as I know, hitherto quite unknown, I am persuaded this Paper, with all its defects, may not be unacceptable to those who feel pleasure in contemplating the varied and beautiful relations between abstract quantities, the wonderful and extensive analogies which every step of our progress in the higher parts of geometry opens to our view.

PROP. I. PORISM. (Tab. XXI. fig. 1.)

A conic hyperbola being given, a point may be found, such, that every straight line drawn from it to the curve, shall cut, in

* See the celebrated account of ancient geometrical works, in the eleventh book of PAPPUS ALEXANDRINUS.

a given ratio, that part of a straight line passing through a given point which is intercepted between a point in the curve not given, but which may be found, and the ordinate to the point where the first mentioned line meets the curve.

Let X be the point to be found, NA the line passing through the given point N, and M any point whatever in the curve: join XM, and draw the ordinate MP; then AC is to CP in a given ratio.

Corollary. This property suggests a very simple and accurate method of describing a conic hyperbola, and then finding its centre, asymptotes, and axes; or, any of these being given, of finding the curve, and the remaining parts.

PROP. II. PORISM.

A conic hyperbola being given, a point may be found, such, that if from it there be drawn straight lines to all the intersections of the given curve, with an infinite number of parabolas, or hyperbolas, of any given order whatever, lying between straight lines, of which one passes through a given point, and the other may be found, the straight lines so drawn, from the point found, shall be tangents to the parabolas, or hyperbolas.

This is in fact two propositions; there being a construction for the case of parabolas, and another for that of hyperbolas.

PROP. III. PORISM.

If, through any point whatever of a given ellipse, a straight line be drawn parallel to the conjugate axis, and cutting the ellipse in another point; and if at the first point a perpendicular be drawn to the parallel, a point may be found, such, that if from it there be drawn straight lines, to the innumerable

intersections of the ellipse with all the parabolas of orders not given, but which may be found, lying between the lines drawn at right angles to each other, the lines so drawn from the point found, shall be normals to the parabolas at their intersections with the ellipse.

PROP. IV. PORISM.

A conic hyperbola being given, if through any point thereof a straight line be drawn parallel to the transverse axis, (and cutting the opposite hyperbolas,) a point may be found, such, that if from it there be drawn straight lines, to the innumerable intersections of the given curve with all the hyperbolas of orders to be found, lying between straight lines which may be found, the straight lines so drawn shall be normals to the hyperbolas at the points of section.

Scholium. The two last propositions give an instance of the many curious and elegant analogies between the hyperbola and ellipse; failing, however, when by equating the axes we change the ellipse into a circle.

PROP. V. LOCAL THEOREM. Fig. 2.

If, from a given point A, a straight line DE moves parallel to itself, and another CS, from a given point C, moves along with it round C; and a point I moves along AB, from H, the middle point of AB, with a velocity equal to half the velocity of DE; then, if AP be always taken a third proportional to AS and BC, and through P, with asymptotes D′E′ and AB, a conic hyperbola be described; also, focus I and axis AB, a conic parabola be described, the *radius vector* from C to M, the

intersection of the two curves, moving round C, shall describe a given ellipse.

PROP. VI. THEOREM.

A common logarithmic being given, and a point without it, a parabola, hyperbola, and ellipse, may be described, which shall intersect the logarithmic and each other in the same points; the parabola shall cut the logarithmic orthogonally; and, if straight lines be drawn from the given point to the common intersections of the four curves, these lines shall be normals to the logarithmic.

PROP. VII. PORISM.

Two points in a circle being given, (but not in one diameter,) another circle may be described, such, that if from any point thereof to the given points straight lines be drawn, and a line touching the given circle, the tangent shall be a mean proportional between the lines so inflected.

Or, more generally, the *square* of the tangent shall have a given ratio to the rectangle under the inflected lines.

PROP. VIII. PORISM. Fig. 3.

Two straight lines AB, AP, (not parallel,) being given in position, a conic parabola MN may be found, such, that if, from any point thereof M, a perpendicular MP be drawn to the one of the given lines nearest the curve, and this perpendicular be produced till it meets the other line in B, and if from B a line be drawn to a given point C, MP shall have to PB together with CB, a given ratio.

Scholium. This is a case of a most general enunciation, which gives rise to an infinite variety of the most curious porisms.

PROP. IX. PORISM. Fig. 4.

A conic hyperbola being given, a point may be found, from which if straight lines be drawn to the intersections of the given curve with innumerable parabolas (or hyperbolas) of any given order whatever, lying between perpendiculars which meet in a given point, the lines so drawn shall cut, in a given ratio, all the *areas* of the parabolas (or hyperbolas) contained by the peripheries and co-ordinates to points thereof, found by the innumerable intersections of another conic hyperbola, which may be found.

This comprehends, evidently, two propositions; one for the case of parabolas, the other for that of hyperbolas. In the former it is thus expressed with a figure.

Let EM be the given hyperbola; BA, AC, the perpendiculars meeting in a given point A: a point X may be found, such, that if XM be drawn to any intersection M of EM with any parabola AMN, of any given order whatever, and lying between AB and AC, XM shall cut, in a given ratio, the area AMNP, contained by AMN and AP, PN, co-ordinates to the conic hyperbola FN, which is to be found; thus, the area ARM shall be to the area RMNP in a given ratio.

PROP. X. PORISM.

A conic hyperbola being given, a point may be found, such, that if from it there be drawn straight lines, to the innumerable intersections of the given curve with all the straight lines drawn through a given point in one of the given asymptotes, the

first mentioned lines shall cut, in a given ratio, the areas of all the triangles whose bases and altitudes are the co-ordinates to a second conic hyperbola, which may be found, at the points where it cuts the lines drawn from the given point.

PROP. XI. PORISM.

A conic hyperbola being given, a straight line may be found, such, that if another move along it in a given angle, and pass through the intersections of the curve with all the parabolas, (or hyperbolas,) of any given order whatever, lying between straight lines to be found, the moving line shall cut, in a given ratio, the areas of the curves described, contained by the peripheries and co-ordinates to another conic hyperbola, that may be found, at the points where this cuts the curves described.

PROP. XII. PORISM.

A conic hyperbola being given, a straight line may be found, along which if another move in a given angle, and pass through any point whatever of the hyperbola, and if this point of section be joined with another that may be found, the moving line shall cut, in a given ratio, the triangles whose bases and altitudes are the co-ordinates to a conic hyperbola, which may be found, at the points where it meets the lines drawn from the point found.

Scholium. I proceed to give one or two examples, wherein areas are cut in a given ratio, not by straight lines, but by curves.

PROP. XIII. PORISM. Fig. 5.

A conic hyperbola being given, if through any of its innumerable intersections with all the parabolas of any order, lying

between straight lines, whereof one is an asymptote, and the other may be found, an hyperbola of any order be described, (except the conic,) from a given origin in the given asymptote perpendicular to the axis of the parabolas, the hyperbola thus described shall cut, in a given ratio, an area (of the parabolas) which may be always found.

If from G (as origin) in AB, one of LM's asymptotes, there be described an hyperbola IC′, of any order whatever, except the first, and passing through M, a point where LM cuts any of the parabolas AM, of any order whatever, drawn from A a point to be found, and lying between AB and AC, an area ACD may be always found, (that is, for every case of AM and IC′,) which shall be constantly cut by IC′, in the given ratio of M : N; that is, the area AMN : NMDC : : M : N.

I omit the analysis, which leads to the following construction and composition.

Construction. Let $\overline{m+n}$ be the order of the parabolas, and $\overline{p+q}$ that of the hyperbolas. Find ϕ a fourth proportional to $\overline{m+n}$, $\overline{q-p}$ and $m+2n$; divide GB in A, so that AR : AG : : q : $p+\phi$; then find π a fourth proportional to M + N, $\phi+p$, and $q-p$, and γ a fourth proportional to q, AG, and $q-p$; and, lastly, θ a fourth proportional to the parameter* of LM, π and M. If, with a parameter equal to $\frac{\overline{m+n}}{m+2n}\times\theta - \frac{M+N}{M}$ of the rectangle τ . AN, and between the asymptotes AB, AC, a conic hyperbola be described, it shall cut the parabola in a point, the co-ordinates to which contain an area that shall be cut by IC′ in the ratio of M : N.

Demonstration. Because AG is divided in R, so that AR : AG

* *i. e.* The constant rectangle or space to which AP . SM is equal.

$:: q : p + \varphi$, and that $\varphi : m + n :: q - p : m + 2n$, AR is equal to $\dfrac{AG \times q}{p + \dfrac{\overline{m+n} \times \overline{q-p}}{m+2n}}$; and, because LM is a conic hyperbola, the rectangle MS . RS, or MS . AP, or AP . $\overline{MP + AR}$ is equal to the parameter, (or constant space,) therefore, this parameter is equal to AP $\times \overline{MP + \dfrac{AG \cdot q}{p + \dfrac{(m+n)(q-p)}{m+2n}}}$

Again, the space ACD is equal to $\frac{m+n}{m+2n}$ of the rectangle AC . CD, since AD is a parabola of the order $m+n$; but (by construction) AC . CD is equal to $\frac{m+n}{m+2n}$ of $\overline{\theta - \frac{M+N}{M} \cdot \tau \cdot AN}$; therefore, ACD $= \theta - \frac{M+N}{M} \cdot \tau \cdot$ AN, of which θ : parameter of LM $:: \pi : M$, and $\pi : M + N :: \varphi + p : q - p$; therefore, $\theta = \dfrac{\text{Par. LM} \times \overline{M+N}}{M(q-p)} \times \left(\dfrac{\overline{m+n} \times \overline{q-p}}{m+2n} + p;\right)$ also, $\tau : q :: AG : q - p$; consequently, ACD $= \dfrac{\text{Par. LM} \times \overline{M+N}}{M(q-p)}$ multiplied by $\left(\dfrac{\overline{m+n} \times \overline{q-p}}{m+2n} + p\right)$ and diminished by $\dfrac{M+N}{M} \times AN \times \dfrac{q \cdot AG}{q-p}$; therefore, transposing $\dfrac{\text{Par. LM} \times \overline{M+N}}{M \times \overline{q-p}} \times \left(\dfrac{\overline{m+n}}{m+2n} \times \overline{q-p} + p\right)$ is equal to ACD $+ \dfrac{\overline{M+N}}{M} \times AN \times \dfrac{q \cdot AG}{q-p}$; and Par. LM will be

equal to $\dfrac{\left(\overline{ACD + \dfrac{M+N}{M} \times AN \times \dfrac{q \cdot AG}{q-p}}\right) \times \dfrac{M}{q-p}}{\left(\dfrac{\overline{m+n}}{m+2n} \times \overline{q-p} + p\right) \times \overline{M+N}}$, that is,

$$\frac{\dfrac{M}{M+N} \times \overline{q-p} \times ACD + q \cdot AN \times AG}{\dfrac{\overline{m+n}}{m+2n} \times \overline{q-p} + p}.$$

Now, it was before demonstrated, that the parameter of LM is equal to $\mathrm{AP} \times \dfrac{\overline{\mathrm{MP} + q \,.\, \mathrm{AG}}}{p + \frac{(m+n)(q-p)}{m+2n}}$. This is therefore equal to $\dfrac{\frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \overline{q-p} \times \mathrm{ACD} + q \,.\, \mathrm{AN} \times \mathrm{AG}}{\frac{m+n}{m+2n} \times \overline{q-p} + p}$, multiplying both by $\dfrac{\overline{m+n} \times \overline{q-p}}{m+2n} + p$, we have $\frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \overline{q-p} \times \mathrm{ACD} + q \,.\, \mathrm{AN} \times \mathrm{AG}$ $= \mathrm{AP} \times \left(\mathrm{MP} \times \left(p + \frac{\overline{m+n} \times \overline{q-p}}{m+2n}\right) + q \,.\, \mathrm{AG}\right)$.

From these equals take $q \,.\, \mathrm{AG} \times \mathrm{AN}$, and there remains $\frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \overline{q-p} \times \mathrm{ACD}$ equal to $\mathrm{AP} \times \mathrm{PM} \times \left(\frac{\overline{m+n} \times \overline{q-p}}{m+2n} + p\right)$ $+ q \,.\, \mathrm{AG} \times \overline{\mathrm{AP} - \mathrm{AN}}$; or, dividing by $q-p$; $\frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}} \times \mathrm{ACD}$ $= \mathrm{AP} \times \overline{\frac{m+n}{m+2n} + \frac{p}{q-p}} \times \mathrm{PM} + \frac{q}{q-p} \times \mathrm{AG} \times \overline{\mathrm{AP} - \mathrm{AN}}$. Now, $\frac{m+n}{m+2n} \times \mathrm{AP} \times \mathrm{PM}$ is equal to the area APM; therefore, the area APM together with $\frac{p}{q-p} \times \mathrm{AP} . \mathrm{PM}$, and $\frac{q}{q-p} \times \mathrm{AG} \times \overline{\mathrm{AP} - \mathrm{AN}}$, or APM with $\frac{p}{q-p} \times \mathrm{AP} . \mathrm{PM} - \frac{q}{q-p} \times \mathrm{AG} \times \overline{\mathrm{AN} - \mathrm{AP}}$, or $\mathrm{APM} + \frac{q}{q-p} \times \mathrm{AP} . \mathrm{PM} - \frac{q}{q-p} \times$ rect. PT is equal to $\frac{\mathrm{M}}{\mathrm{M}+\mathrm{N}}$ $\times$ACD. Now, IC′ is an hyperbola of the order $p+q$; therefore, its area is $\frac{p}{p-q} \times$ rect. GH . MH. But q is greater than p; therefore, $\frac{p}{p-q}$ is negative, and $\frac{p \times \mathrm{GH} . \mathrm{HM}}{q-p}$ is the area MHKC′; and the area NTKC′ is equal to $\frac{p}{q-p} \times \mathrm{GT} \times \mathrm{TN}$; therefore, MNTH is equal to (MHKC′—NTKC′), or to $\frac{p}{q-p} \times \overline{\mathrm{GH} . \mathrm{MH} - \mathrm{GT} . \mathrm{TN}}$. From these equals take the common rectangle AT, and there

remains the area MPN, equal to $\frac{p}{q-p} \times AP \times MP - \frac{q}{q-p} \times PT$; which was before demonstrated to be, together with APM, equal to $\frac{M}{M+N} \cdot ACD$. Wherefore MPN, together with APM, that is, the area AMN is equal to $\frac{M}{M+N} \cdot ACD$; consequently, AMN : ACD :: M : M + N; and (*dividendo*) AMN : NMDC :: M : N. An area has therefore been found, which the hyperbola IC′ always cuts in a given ratio.

Wherefore, a conic hyperbola being given, *&c. Q. E. D.*

Scholium. This proposition points out, in a very striking manner, the connection between all parabolas and hyperbolas, and their common connection with the conic hyperbola. The demonstration which I have given is much abridged; and, to avoid circumlocution, algebraic symbols, and even ideas, have been introduced: but, by attending to the several steps, any one will easily perceive that it may be translated into geometrical language, and conducted upon purely geometrical principles, if any numbers be substituted for m, n, p, and q; or if these letters be made representatives of lines, and if conciseness be less rigidly studied.

PROP. XIV. THEOREM.

A common logarithmic being given, if from a given point, as origin, a parabola (or hyperbola) of any order whatever be described, cutting, in a given ratio, a given area of the logarithmic, the point where this curve meets the logarithmic is always situated in a conic hyperbola, which may be found.

Scholium. This proposition is, properly speaking, neither a porism, a theorem, nor a problem. It is not a theorem, because something is left to be found, or, as PAPPUS expresses

it, there is a deficiency in the hypothesis: neither is it a porism; for the theorem, from which the deficiency distinguishes it, is not local.

PROP. XV. PORISM. Fig. 6.

A conic hyperbola being given, two points may be found, from which if straight lines be inflected, to the innumerable intersections of the given curve with parabolas or hyperbolas, of any given order whatever, described between given straight lines, and if co-ordinates be drawn to the intersections of these curves with another conic hyperbola, which may be found, the lines inflected shall always cut off areas that have to one another a given ratio, from the areas contained by the co-ordinates.

Let X and Y be the points found; HD the given hyperbola, FE the one to be found; ADC one of the curves lying between AB and AG, intersecting HD and FE: join XD, YD; then the area AYD : XDCB in a given ratio.

PROP. XVI. PORISM. Fig. 7.

If, between two straight lines making a right angle, an infinite number of parabolas, of any order whatever, be described, a conic parabola may be drawn, such, that if tangents be drawn to it at its intersections with the given curves, these tangents shall always cut, in a given ratio, the areas contained by the given curves, the curve found, and the axis of the given curves.

Let AMN be one of the given parabolas; DMO the parabola found, and TM its tangent at M: ATM shall have to TMR a given ratio.

PROP. XVII. PORISM.

A parabola of any order being given, two straight lines may be found, between which if innumerable hyperbolas of any order be described, the areas cut off by the hyperbolas and the given parabola at their intersections, shall be divided, in a given ratio, by the tangents to the given curve at the intersections; and, conversely, if the hyperbolas be given, a parabola may be found, *&c. &c.*

PROP. XVIII. PORISM.

A parabola of any order $(m+n)$ being given, another of an order $(m+2n)$ may be found, such, that the rectangle under its ordinate and a given line, shall have always a given ratio to the area (of the given curve) whose abscissa bears to that of the curve found a given ratio.

Example. Let $m=1$, $n=1$, and let the given ratios be those of equality; the proposition is this; a conic parabola being given, a semicubic one may be found, such, that the rectangle under its ordinate and a given line, shall be always equal to the area of the given conic parabola, at equal abscissæ.

Scholium. A similar general proposition may be enunciated and exemplified, with respect to hyperbolas; and, as these are only cases of a proposition applying to all curves whatsoever, I shall take this opportunity of introducing a very simple, and, I think, perfectly conclusive demonstration, of the 28th lemma, *Principia*, Book I. "*that no oval can be squared.*" It is well known, that the demonstration which Sir ISAAC NEWTON gives of this lemma, is not a little intricate; and, whether from this difficulty, or from some real imperfection, or from a very natural

wish not to believe that the most celebrated *desideratum* in geometry must for ever remain a *desideratum*, certain it is, that many have been inclined to call in question the conclusiveness of that proof.

Let AMC be any curve whatever, (fig. 8.) and D a given line; take in ab a part ap, having to AP a given ratio, and erect a perpendicular pm, such, that the rectangle pm . D shall have to the area APM a given ratio: it is evident that m will describe a curve amc, which can never cut the axis, unless in a. Now, because pm is proportional to $\frac{\text{APM}}{\text{D}}$, or to APM, pm will always increase, *ad infinitum*, if AMC is infinite; but, if AMC stops or returns into itself, that is, if it is an oval, pm is a *maximum* at b, the point of ab corresponding to B in AB; consequently, the curve *amc stops short*, and is *irrational*. Therefore pm, its ordinate, has not a finite relation to ap, its abscissa; But ap has a given ratio to AP; therefore pm has not a finite relation to AP, and APM has a given ratio to pm; therefore it has not a finite relation to AP, that is, APM cannot be found in finite terms of AP, or is incommensurate with AP; wherefore, the curve AMB cannot be squared. Now, AMB is any oval; wherefore no oval can be squared. By an argument of precisely the same kind, it may proved, that the *rectification*, also, of every oval is impossible. Wherefore, *&c. Q. E. D.*

I shall subjoin three problems, that occurred during the consideration of the foregoing propositions. The first is an example of the application of the porisms to the solution of problems. The second gives, besides, a new method of resolving one of the most celebrated ever proposed, KEPLER's problem; and

the last presents to our view a curve before unknown, (at least to me,) as possessing the singular property of a constant tangent.

PROP. XIX. PROBLEM. Fig. 9.

A common logarithmic being given, to describe a conic hyperbola, such, that if from its intersection with the given curve a straight line be drawn to a given point, it shall cut a given area of the logarithmic in a given ratio. The analysis leads to this

Construction. Let BME be the logarithmic, G its modula; AB the ordinate at its origin A; let C be the given point; ANOB the given area; M : N the given ratio: draw BQ parallel to AN; find D a fourth proportional to M, the rectangle BQ . OQ, and M + N. From AD cut off a part AL, equal to AC together with twice G; at L, make LH perpendicular to AD, and, between the asymptotes AL, HL, with a parameter * twice $(\overline{D + 2 . AB . G})$ describe a conic hyperbola: it is the curve required.

PROP. XX. PROBLEM. Fig. 10.

To draw through the focus of a given ellipse, a straight line that shall cut the area of the ellipse in a given ratio.

Construction. Let AB be the transverse axis, EF the semiconjugate; E, of consequence, the centre; C and L the foci. On AB describe a semicircle. Divide the quadrant AK in the given ratio in which the area is to be cut, and describe the cycloid GMR, such, that the ordinate PM may be always a

* Or constant rectangle.

fourth proportional to the arc OQ, the rectangle AB × 2 . FE, and the line CL; this cycloid shall cut the ellipse in M, so that, if MC be joined, the area ACM shall be to CMB : : M : N.

Demonstration. Let AP $= x$, PM $= y$, AC $= c$, AB $= a$, and 2 . EF $= b$; then, by the nature of the cycloid GMR, $-$PM : $\overline{\text{OQ}}$: : 2 . FE × AB : cL, and QO $=$ AO $-$ AQ $=$ (by const.) $\frac{M}{M+N} \times \overline{AK} - AQ$; also, CL $=$ AB $-$ 2 . AC, since AC $=$ LB. Therefore, $-\text{PM} : \frac{M}{M+N} \times \text{AK} - \text{AQ} : : \text{AB} \times 2 . \text{EF} : \text{AB} - 2 . \text{AC}$; or $-y : \frac{M}{M+N} \times \text{arc} . 90° - \text{arc.vers.sin.}x : : ab : a - 2c$; therefore, $-y\,(a-2c)$ or $+y\,(2c-a) = ab \times \overline{\left(\frac{M}{M+N} \times \text{arc}.90° - \text{arc.v.s.}x\right)}$ and, by transposition,

$ab \times \text{arc} . \overline{\text{v.s.}}\, x + y\,(2c - a) = \frac{ab . M}{M+N} \times \text{arc} . 90°$. To these equals, add $2y\,(x - x) = 0$, and multiply by -1; then will $ab \times \text{arc}.\overline{\text{v.s.}}\,x + (2x - a)\,y - 2y\,(x - c) = \frac{M}{M+N} \times ab \times \text{arc}.90°$, of which the fourth parts are also equal; therefore,

$$\frac{ab \times \text{arc}.\overline{\text{v.s.}}\,x}{4} + \frac{(2x-a)\,y}{4} - \frac{y}{2}\,(x-c) = \frac{ab}{4} \times \frac{M}{M+N} \times \text{arc} . 90°.$$

Now, because AFB is an ellipse, $y^2 = \frac{b^2}{a^2} \times \overline{ax - x^2}$, and $y = \frac{b}{a}\sqrt{ax - x^2}$; therefore, $\frac{ab \times \text{arc}.\overline{\text{v.s.}}\,x}{4} + \frac{(2x-a)}{4} \times \frac{b}{a}\sqrt{ax - x^2} - \frac{y}{2}\,(x - c) = \frac{ab}{4} \times \frac{M}{M+N} \times \text{arc}.90°$. Multiply both numerator and denominator of the first and last terms by a; then will $\frac{b}{a} \times \frac{a^2}{4} \times \text{arc} . \overline{\text{v.s.}}\,x + \frac{\overline{2x - a}}{4} \times \frac{b}{a}\sqrt{ax - x^2} - \frac{y}{2}\,(x - c) = \frac{b}{a} \times \frac{a^2}{4} \times \frac{M}{M+N} \times \text{arc}.90°$. Now, the fluxion of an arc whose versed sine is x and radius $\frac{a}{2}$, is equal to $\frac{a\dot{x}}{2\sqrt{ax - x^2}}$, which is also the fluxion of the arc whose sine is $\sqrt{\frac{x}{a}}$ and radius unity;* wherefore,

* The semi-transverse is supposed unity, through this demonstration.

$\frac{b}{a} \times \left(\frac{a^2}{4} \times \text{arc} \, . \, \sin. \sqrt{\frac{x}{a}} + \frac{\overline{2x - a}}{4} \times \sqrt{ax - x^2}\right) - \frac{y}{2}(x - c)$ is equal to $\frac{b}{a} \times \frac{a}{4} \times \frac{M}{M + N} \times \text{arc} \, . \, 90^\circ$; and, by the quadrature of the circle, $\frac{a^2}{4} \times \text{arc} \, . \, \sin. \sqrt{\frac{x}{a}} + \frac{\overline{2x - a}}{4} \times \sqrt{ax - x^2}$, is the area whose abscissa is x; consequently, the semicircle's area is $\frac{a^2}{4} \times \text{arc} \, . \, 90^\circ$. But the areas of ellipses are, to the corresponding areas of the circles described on their transverse axes, as the conjugate to the transverse; therefore $\frac{b}{a} \times \left(\frac{a^2}{4} \times \text{arc} \, . \, \sin. \sqrt{\frac{x}{a}} + \frac{\overline{2x - a}}{4} \times \sqrt{ax - x^2}\right)$ is the area whose abscissa is x, of a semi-ellipse whose axes are a and b; and, consequently, $\frac{b}{a} \times \frac{a^2}{4} \times \text{arc} \, . \, 90^\circ$ is the area of the semi-ellipse. Wherefore, the area APM $- \frac{y}{2}(x - c)$ is equal to $\frac{M}{M + N}$ of AMFB. But $\frac{y}{2}(x - c) \left(= \frac{PM}{2} \times \overline{AP - AC} = \frac{PM}{2} \times PC\right)$ is the triangle CPM; consequently, APM $-$ CPM, or ACM, is equal to $\frac{M}{M + N} \times$ AMFB; and ACM : AMFB :: M : M + N; or (*dividendo*) ACM : CMFB :: M : N; and the area of the ellipse is cut in a given ratio by the line drawn through the focus. *Q. E. D.*

Of this solution it may be remarked, that it does not assume as a postulate the description of the cycloid, but gives a simple construction of that curve, flowing from a curious property, whereby it is related to a given circle. This cycloid too gives, by its intersection with the ellipse, the point required, directly, and not by a subsequent construction, as Sir I. NEWTON's does. I was induced to give the demonstration, from a conviction that it is a good instance of the superiority of modern over ancient

analysis; and in itself, perhaps, no inelegant specimen of algebraic demonstration.

PROP. XXI. PROBLEM. Fig. 11.

To find the Curve whose Tangent is always of the same Magnitude.

Analysis. Let MN be the curve required, AB the given axis, SM a tangent at any point M, and let d be the given magnitude; then, SM . q . $=$ SP . q . $+$ PM . q . $= d^2$; or, $y^2 + \frac{\dot{y}^2 \dot{x}^2}{\dot{y}^2}$ $= d^2$, and $\frac{\dot{x}^2}{\dot{y}^2} = \frac{d^2 - y^2}{y^2}$; therefore, $\dot{x} = \frac{\dot{y}}{y} \times \sqrt{d^2 - y^2}$. In order to integrate this equation, divide $\frac{\dot{y}}{y}\sqrt{d^2 - y^2}$ into its two parts, $\frac{d^2 \dot{y}}{y\sqrt{d^2 - y^2}}$ and $\frac{-y\dot{y}}{\sqrt{d^2 - y^2}}$.

To find the fluent of the former,

$$\frac{d^2\dot{y}}{y\sqrt{d^2-y^2}} = \frac{d^2\dot{y}}{y} \times \frac{\left(1 + \frac{d}{\sqrt{d^2 - y^2}}\right)}{d + \sqrt{d^2 - y^2}} = \frac{-d \times \left(-\frac{d\dot{y}}{y^2} - \frac{d^2\dot{y}}{y^2\sqrt{d^2-y^2}}\right)}{\frac{d + \sqrt{d^2 - y^2}}{y}}$$

$$= -\frac{d \times \text{fluxion of } \frac{d + \sqrt{d^2 - y^2}}{y}}{\frac{d + \sqrt{d^2 - y^2}}{y}};$$ therefore, the fluent of $\frac{d^2\dot{y}}{y\sqrt{d^2 - y^2}}$ is $-d \times$ hyp. log. $\frac{d + \sqrt{d^2 - y^2}}{y}$, and the fluent of the other part, $\frac{-y\dot{y}}{\sqrt{d^2 - y^2}}$ is $+\sqrt{d^2 - y^2}$; therefore, the fluent of the aggregate $\frac{\dot{y}}{y}\sqrt{d^2 - y^2}$ is $\sqrt{d^2 - y^2} - d \times$ h. l. $\frac{d + \sqrt{d^2 - y^2}}{y}$, or $\sqrt{d^2 - y^2} +$ $d \times$ h. l. $\frac{y}{d + \sqrt{d^2 - y^2}}$; a final equation to the curve required.

Q. E. I.

I shall throw together, in a few corollaries, the most remarkable things that have occurred to me concerning this curve.

Corollary 1. The subtangent of this curve is $\sqrt{d^2 - y^2}$.

Corollary 2. In order to draw a tangent to it, from a given point without it; from this point as pole, with radius equal to d, and the curve's axis as directrix, describe a *conchoid of Nicomedes:* to its intersections with the given curve draw straight lines from the given point; these will touch the curve.

Corollary 3. This curve may be described (organically) by drawing one end of a given flexible line or thread along a straight line, whilst the other end is urged by a weight towards the same straight line. It is, consequently, the *curve of traction* to a straight line.

Corollary 4. In order to describe this curve from its equation: change the one given above, by *transferring* the axes of its co-ordinates: it becomes (y being $=$ P′M and $x =$ AP′) $y = \sqrt{d^2 - x^2} + d \times \text{h. l.} \frac{x}{d + \sqrt{d^2 - x^2}}$; which may be used with ease, by changing the hyperbolic into the tabular logarithm.

Thus then, the common logarithmic has its *subtangent* constant; the conic parabola, its *subnormal;* the circle, its *normal:* and the curve which I have described in this proposition, its *tangent.*

Edinburgh, 1797.

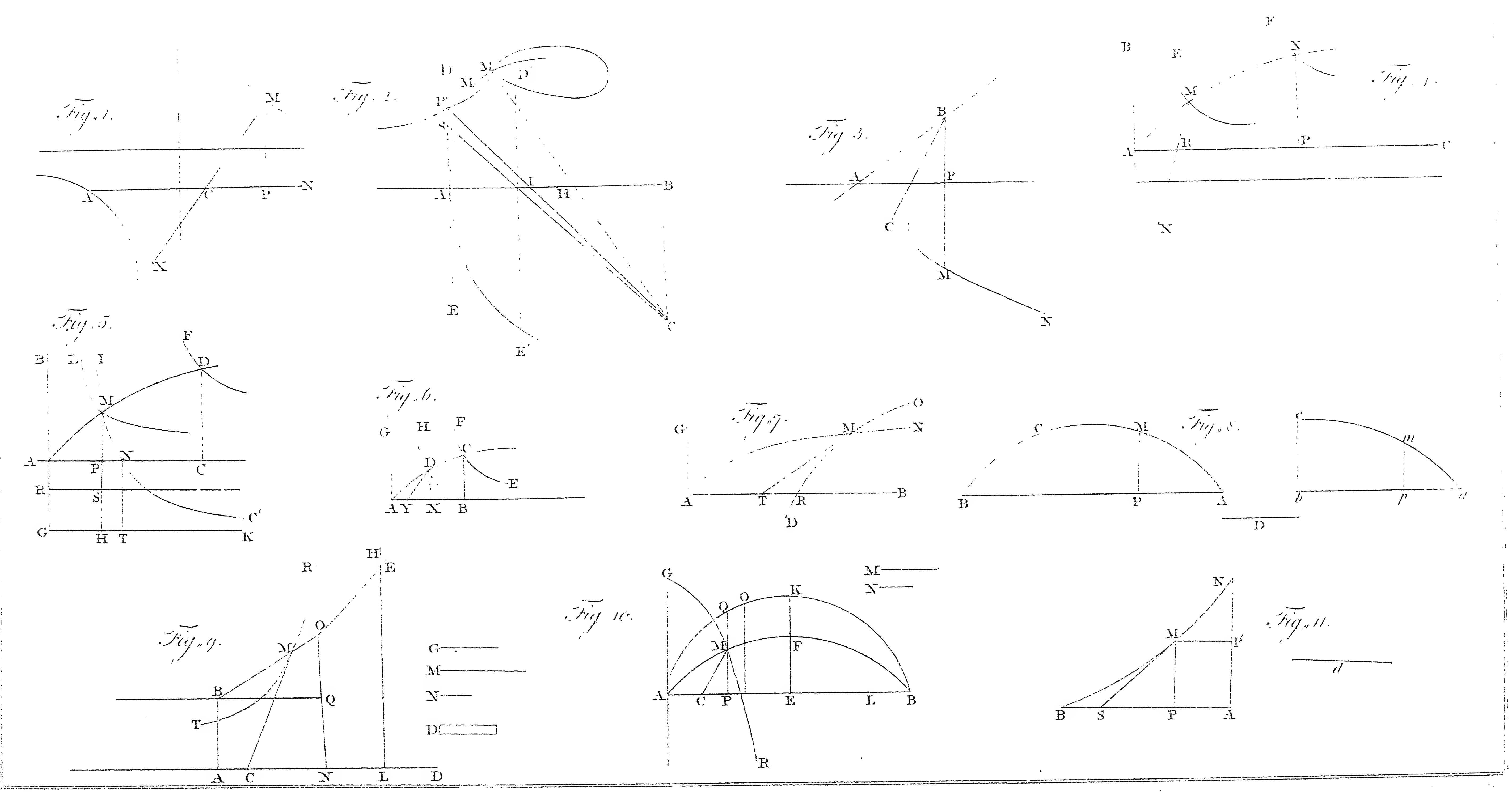


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.

XVIII. *Observations of the diurnal Variation of the Magnetic Needle, in the Island of St. Helena; with a Continuation of the Observations at Fort Marlborough, in the Island of Sumatra. By* John Macdonald, *Esq. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read May 24, 1798.

SIR, Edinburgh, September 30, 1797.

On my arrival in England, I had the honour of observing to you, that I had taken some observations of the diurnal variation of the magnetic needle, in the island of St. Helena. I am to apologize to you for having, till this period, omitted furnishing you with these, and with a continuation of those formerly taken in the island of Sumatra. The meridian was laid off by means of an apparatus brought from Bencoolen; and the requisite allowance made for the alteration of the sun's declination during the operation. The meridian-plate remains firmly set in a pillar of teak-wood, well fixed, for the use of navigators; who, by applying a compass-card to it, will find the variation more readily, and correctly, than by amplitude or azimuth. A short residence at St. Helena, arising from the sudden departure of the fleet to which the ship I was in belonged, has prevented the observations from being as numerous as I could wish. Their agreement, however, indicates that fifty-eight observations are sufficient for affording such conclusions

as philosophy may draw; and tends to confirm some inferences stated in a former Paper, containing similar observations taken in the East Indies. By adding the mean of the morning and afternoon observations, at St. Helena, and taking the half, the general variation, in the month of November, 1796, appears to have been $15^\circ\ 48'\ 34''\frac{1}{2}$ west: and, by subtracting the medium diurnal afternoon variation, from the medium diurnal morning, the vibrating variation proves to be $3'\ 55''$. It appears, that the magnetic needle is stationary from about six o'clock in the evening till six o'clock in the morning; when it commences moving, and the west variation increases, till it amounts to its maximum, about eight o'clock; diminishing afterwards, till it becomes stationary. Here, the same cause seems to operate as at Bencoolen, with a modification of effect, proportioned to the relative situations of the southern magnetic poles, and the places of observation. At the apartments of the Royal Society, this species of variation is found to increase, from seven o'clock in the morning till two o'clock in the afternoon. If the variation is east, in the northern hemisphere in the East Indies, I conceive that the diurnal variation will increase towards the afternoon, remain some time stationary, and diminish before the succeeding morning: if the general variation is west, in that quarter, the reverse may be the case. The quantity of the diurnal variation is greater in Britain than at St. Helena, or at Bencoolen. This will naturally arise from this country's being more contiguous to its affecting poles, than those islands situated near the equator. It were to be wished, that observations were taken in as many situations as possible, similarly situated in the opposite hemispheres, on the lines of no variation. A greater degree of dip might be found, and conclusions might

be deduced, that would tend considerably to illustrate this curious and interesting subject, as yet involved in conjecture and uncertainty. I frequently, while at Bencoolen, observed that the needle did not retain the same level, but was sometimes depressed, and sometimes elevated, six or eight minutes. I paid little attention to this, ascribing it to a minute alteration in the position of the point of the socket over the pivot. I observed, sometimes, a similar difference of level in the position of the needle at St. Helena, without being able to account for it. It may be possible, that the dip of the needle is subject to a diurnal variation in its vertical movement. I have perused such publications as have appeared on magnetism for some time past: they state no theory of this obscure science, more rational, or satisfactory, than that left us by the celebrated HALLEY.

I have the honour to be, *&c.*

JOHN MACDONALD.

In the following observations, F. means fair; A. after; R. rain; N. night; L. lightning; C. cloudy; S. sky; T. thunder; m. much; o. overcast; s. serene; W. windy; l. little; a, the indefinite article; E. or W. east or west variation: a figure, as 7, over the degree, means that the observation was taken then, and not at the usual hour.

Observations of the diurnal Variation of the Magnetic Needle, in the Island of St. Helena.

1796.	Time. 8 A.M. ° ′	Thermometer.	Time. 12. ° ′	Thermometer.	Time. 2 P.M. ° ′	Thermometer.	Time. Various Hours.	° ′	Thermometer.	Weather. 1st Observation.	Weather. 2d Observation.	Weather. 3d Observation.	Weather. 4th Observation.
	W.		W.		W.			W.					
Oct. 28	15 50	-	-	-	15 48								
29	15 52	-	-	-	15 48								
30	15 51	69	15 51	70	15 48	71	-	-	-	s. sunshine	W.	s. sunshine	
31	15 50	69	15 48	71	15 46	71¼	5 P. M.	15 49	70½	C.	R.	s. ditto	C.
Nov. 1	15 49	68½	15 48	70	15 46	71	5½ P.M.	15 47	70	R.	F. W.	s. ditto	ditto
2	15 48	67½	15 47	68¼	15 45	70	6½ P.M.	15 48	70	ditto	R.	s. ditto	s. C.
4	15 50	68	15 50	68½	15 46	70¼	5¾ P.M.	15 48	70½	s. sunshine	ditto	sunsh. and show.	R.
5	15 51	68½	15 48	69	15 47	70½	6¼ P.M.	15 47	69½	C.	a l. R.	ditto. F.	C.
7	15 51	69	15 48	70	15 48	71	4 P. M.	15 49	70	R.	W.	s. C.	ditto
8	15 51	68	15 45	69	15 48	70½	7 A. M.	15 49	67½	R.	sunshine W.	s. sunshine	R. and W.
9	15 50	68	15 46	70	15 47	70	5½ A.M.	15 48	68	C.	R.	s. ditto	s. sunshine
10	15 51	68⅔	15 48	70½	15 46	71½	6 A. M.	15 50	68	ditto	s. sunshine	a l. C.	C.
11	15 52	69½	15 49	70½	15 47	71	6 A. M.	15 48	68½	a l. W.	s. ditto	C.	s. C.
12	15 52	69	15 48	70⅔	15 46	71¼	5¼ A.M.	15 49	68	s. C.	C.	s. sunshine	s. ditto
13	15 50	69	10 o'clock 15 46	70½	-	-	7 A. M.	15 49	69	W.	W.	-	s. sunshine
Nov. 10	-	-	-	-	-	-	6 P. M.	15 48	70½				
10	-	-	-	-	-	-	6½	15 49					
10	-	-	-	-	-	-	7 P. M.	15 49	70				
11	-	-	-	-	-	-	6½ P.M.	15 49	70½				

Continuation of the Observations of the diurnal Variation of the Magnetic Needle, at Fort Marlborough, in Sumatra.

1795.	Time. 7½ A. M. ° ′	Thermometer.	Time. 12. ° ′	Thermometer.	Time. 4½ P. M. ° ′	Thermometer.	Time. Various Hours.	° ′	Thermometer.	Weather. 1st Observation.	Weather. 2d Observation.	Weather. 3d Observation.	Weather. 4th Observation.
	E.		E.		E.			E.					
April 6	1 7	84	1 8	83	1 8	34	4 in the morning.	1 8	77	s. sunshine -	a l. W. -	F. W. -	a s. N.
7	-	-	-	-		-	4 in the morning.	1 8	77	- -	- -	- -	a s. ditto
10	1 6	77	1 9	83	1 10	84	10 P. M.	1 9	77⅓	s. a l. W. -	sunshine, a l. W.	s. sunshine -	a s. ditto
11	1 6	75⅔	1 8	82	1 9	84	- -	-	-	s. sunshine -	s. sunshine -	s. ditto	
13	1 6	76	-	-	1 11	85	5 in the morning.	1 8	78	s. ditto -	s. ditto -	s. ditto -	a s. morning
17	1 6	77	1 8	84	-	-	8 P. M.	1 8	82	s. ditto -	s. ditto -	- -	a s. N.
18	1 7	79	-	-	-	-	- -	-	-	s. ditto			
23	1 6	76	1 12	78	1 9	79	- -	-	-	F. A. m. R.	a l. C. A. T.	C. A. R.	
27	-	-	-	-	1 9	83	- -	-	-	- -	- -	s. and C.	
29	-	-	-	-	-	-	11 P. M.	1 9	79	- -	- -	- -	L. F.
May 2	-	-	-	-	1 10	84	- -	-	-	- -	- -	C. distant T.	
5	1 7	77	-	-	1 10	33⅔	11½ P. M.	1 8	79	F. distant T.	- -	a l. C. distant T.	a s. N.
6	-	-	-	-	-	-	12 P. M.	1 8	78	- -	- -	- -	a W. N.
7	1 8	77	1 8	81	1 8	81½	- -	-	-	C. A. R. -	C. -	C.	
11	1 7	78	1 11	85	1 10	79½	12 P. M.	1 8	78	s. sunshine	s. sunshine -	C. - -	a s. C. N.
12	1 7	77	1 7	84	1 9	84½	10½ P. M.	1 9	79	s. ditto -	s. ditto -	C. - -	s. distant T.
13	1 7	78	-	-	-	-	12 P. M.	1 7	75⅔	s. a l. W. -	- -	- -	C. A. R.
14	1 7	78¼	1 7	81	1 10	82	12 P. M.	1 8	78	s. distant T.	W. A. C. -	s. sunshine -	a s. N.

1795.	Time. 6 A.M. ° '	Thermom.	Time. 12. ° '	Thermom.	Time. 3½ P.M. ° '	Thermom.	Time. Various Hours.	° '	Thermom.	Weather. 1st Observation.	Weather. 2d Observation.	Weather. 3d Observation.	Weather. 4th Observation.
	E.		E.		4 E.								
Aug. 11	1 8	$77\frac{1}{2}$	1 8	83	1 9	83	- -	-	-	a s. morning -	s. sunshine -	s. sunshine	
13	1 8	74	-	-	1 9	82	11 P.M.	1 7	77	a s. ditto -	a l. C. -	a l. C. -	a s. C. N.
Sept. 23	-	-	-	-	1 11	$82\frac{1}{2}$	- -	-	-	- -	- -	sunshine, a l. W.	
24	1 7	76	1 9	84	1 10	84	- -	-	-	s. sunshine -	sunshine, a l. W.	ditto ditto	
Oct. 13	1 6	75	1 9	82	1 12	$82\frac{1}{2}$	$10\frac{1}{4}$ P.M.	1 8	77	s. and F. -	s. sunshine -	s. ditto -	a s. C. N.
14	1 7	$74\frac{1}{2}$	1 10	82	1 11	84	- -	-	-	s. sunshine -	s. sunshine -	s. ditto	
Nov. 7	-	-	-	-	3 1 11	77	- -	-	-	- -	- -	- -	C. A. m. R.
16	-	-	-	-	1 11	83	- -	-	-	- -	- -	s. ditto	
26	-	-	-	-	4 1 10	-	$11\frac{3}{4}$ P.M.	1 8	$76\frac{1}{2}$	- -	- -	s. ditto -	a s. C. N.
Dec. 5	1 6	75	-	-	1 12	82	$9\frac{3}{4}$ P.M.	1 8	77	- -	- -	distant T. -	an o. S. with m. R.
8	-	-	-	-	1 11	82	- -	-	-	- -	- -	s. sunshine	
14	7 1 6	76	-	-	-	-	- -	-	-	F. A. m. R. in the N.			
21	-	-	-	-	3 1 12	82	- -	-	-	- -	- -	s. sunsh. dist. T.	
28	-	-	-	-	-	-	12 P.M.	1 8	77	- -	- -	- -	a fine N.
31	-	-	-	-	1 11	82	- -	-	-	- -	- -	s. and C.	
1796.													
Jan. 12	1 6	74	-	-	-	-	- -	-	-	A. F. C. N.			
19	-	-	-	-	1 10	83	- -	-	-	- -	- -	s. sunshine	
Feb. 1	1 7	75	-	-	-	-	- -	-		F. A. R.	- -		
27	-	-	-	-	1 11	84	- -	-	-	- -	- -	s. sunshine	
Mar. 2	-	-	-	-	1 10	86	- -	-	-	- -	- -	s. ditto	
22	1 7	75	-	-	1 11	84	- -	-	-	A. F. C. M. -	- -	s. ditto	
Apr. 6	1 9	74	-	-	1 12	82	- -	-	-	A. F. C. M.	- -	s. ditto	
19	7 1 7	76	-	-	1 11	-	12 P.M.	1 8	$76\frac{1}{4}$	a F. sunsh. morn.	- -	s. sunshine	a fine N.
May 13	7 1 5	77	-	-	-	-	- -	-	-	a ditto ditto			
June 30	-	-	-	-	1 10	82	- -	-	-	- -	- -	s. sunshine	

XIX. *On the Corundum Stone from Asia. By the Right Hon.* Charles Greville, *F. R. S.*

Read June 7, 1798.

Analysi crystallorum, tam ejusdem quam diversæ figuræ, multum lucis scientia expectat. *Bergman. Opusc. de Terra Gemmarum.*

Having contributed to bring into notice the mineral substance from the East Indies which is generally called Adamantine Spar, I beg leave to lay before the Royal Society the following account of its history and introduction.

About the year 1767 or 1768, Mr. William Berry, a very respectable man, and an eminent engraver of stone, at Edinburgh, received from Dr. Anderson, of Madras, a box of crystals, with information of their being the material used by the natives of India, to polish crystal, and all gems but diamonds. Mr. Berry found that they cut agate, cornelian, &c.; but, in his minute engraving of figures, upon seals, &c. the superior hardness of the diamond appeared preferable; and its dispatch compensated for the price: the crystals were therefore laid aside, as curiosities. Dr. Black ascertained their being different from other stones observed in Europe, and their hardness attached to them the name of Adamantine Spar. My friend Colonel Cathcart sent me its native name, *Corundum*, from India, with some specimens, given to him by Dr. Anderson, in 1784, which I distributed for analysis.

When the native name was obtained, it appeared, from Dr. WOODWARD's Catalogue of Foreign Fossils, published about the year 1719, that the same substance had been sent to him from Madras, by his correspondent Mr. BULKLEY.

In his first Catalogue of Foreign Fossils, p. 6. ζ. 17. "*Nella* "*Corivindum* is found in fields where the rice grows: it is com- "monly thrown up by field rats, and used, as we do emery, "to polish iron."

Page 11. λ. 13. "*Tella Convindum*, Fort St. George, Mr. "BULKLEY. 'Tis a talky spar, grey, with a cast of green: it "is used to polish rubies and diamonds."

In Dr. WOODWARD's additional Catalogue of Foreign Fossils, published in 1725, p. 6. ζ. 10. "*Nella Corivendum* is found "by digging at the foot or bottom of hills, about 500 miles to the "southward of this place. They use it as emery, to clean arms, "&c. it serves also to grind rubies, by making it like hard "cement, by the help of stick-lac mixt with it. East India. Mr. "BULKLEY."—These, with a few others in WOODWARD's Catalogues, are the only instances by which any author, prior to 1768, appears to have noticed this substance.

This information being unsatisfactory, and every appearance of the stone indicating it to be part of a stratum, I wrote repeatedly to friends in India, to ascertain, if possible, the situation of the rock, and, if near the sea, to send a considerable quantity, as ballast, with a view of applying it to cut and polish granites, porphyry, and other stones, which the high price of cutting and polishing excluded from useful or ornamental work. But my inquiries at Madras were fruitless: by some I was assured it came from Guzarat. From Bombay I obtained no satisfactory information. At last, in the year

1793, I obtained a satisfactory account. Sir CHARLES OAKLEY was disposed to oblige me: he was then Governor of Madras; and his success is due to the activity and judgment of Mr. GARROW.

Mr. GARROW knew how difficult it was to avoid the causes of my failure, from every Hindoo being occupied by the duty of his cast; scarcely thinking on any thing else, and, whenever his interest is concerned, being suspicious and reserved. Mr. GARROW, in the first place, ascertained the cast connected with Corundum to be the venders of glass *bangles;* that they used it in their business, and sold it to all other casts. This cast of natives, at all times, had free access to every part of Tippoo's country; nor, until the districts about Permetty were ceded to the English, could it be procured in any other way. Mr. GARROW depended on his personal inspection; the particulars are contained in the following letter, communicated to me by Sir CHARLES OAKLEY.

Sir CHARLES OAKLEY, *Bart.*

SIR, Tritchinopoly, 10th Nov. 1792.

"I derived so little satisfaction from the various accounts "given me of the Corundum, from the indifference of the na- "tives to every subject in which they are not immediately inte- "rested, that I resolved to ascertain the particulars I wished "to know, on the spot where the stone is found. The glass- "men agreed in one material circumstance, that the place was "not far from Permetty: in other particulars they disagreed, "apparently with intention to mislead.

"It is near a fortnight since I dispatched a servant I could

"depend on to Permetty, with one of these people, who, on "his arrival there, probably through fear of his cast, said he "knew no farther. My servant persevered, and informed me "he had found the place I wished to see.

"I arrived at Permetty, by the route of Namcul, the 6th; "and, learning that the distance to the spot was about $3\frac{1}{2}$ hours "or 14 miles, I left Permetty in time to arrive there about "sunrise the next morning. At this time no person but my "servant was present, and, from a continued excavation at "different depths, from 6 to 16 feet, in appearance like a wa-"ter-course, running in length about a mile and a half east "and west, over the brow of a very rising ground, I saw at "once the place from which the stone was procured. The "prodigious extent that at different times appears to have been "dug up, with the few people employed, shews that it has been "a business of ages.

"The ground through which the vein of excavation runs, "and of course the mineral, commands one of the finest and "most extensive prospects it is possible to conceive. The sur-"face of the ground is covered with innumerable fine alabaster "stones, and a variety of small shrubs, but not a tree sufficient "to shelter my palanquin.

"There is not the appearance of an habitation within three "quarters of a mile. The nearest village is called Condrastra "Pollam. In this village are about 90 small thatched houses: "among these are 5 families, who, in descent by prescriptive "right, are the miners, and dig in the pits. The nearest place "of any consequence, in RENNELL's Map, is Caranel, on the "south side the Cavery. The distance of the pits from the "river is above 4 miles; but the ground between prevent its

" being seen in a direct line. A fine view of the river is seen " near Erode; which fort, as well as Sankerdroog, are plainly " visible with the naked eye, as is also the Coimbitoor country, " south and west of the river, to an immense extent.

" I procured, at Permetty, a cadjan from the Bramin manager " to the head man of the Pollam; which, on my arrival at the " pits, I sent to him; and soon after three of the miners came " from the Pollam, with their implements, and families follow- " ing with provisions. As they came up, they inquired of my " servant how they were to address me, having never seen an " European before.

" I followed them into a pit, in the line of the excavation, above " 14 feet from the ground-level. The instrument they used " is a very heavy iron crow, ending in a broad point, with a " straight wooden handle, clampt with iron. The soil they cut " through is of different colours, but composed chiefly of a " gritty granite; and, at the depth of seven feet, are layers of " a substance not unlike dried pitch, which crumbles into small " flakes when taken out. With considerable labour, the miners, " with the points of their crows, cut out several pieces of " the strata, of some pounds weight each; and, when a consi- " derable quantity was broken off, it was carried up and crush- " ed to pieces, with great force, by the iron crow. Among " these broken lumps, the Corundum stone is found; but in " many of the pieces there was none. The mode of getting it, " made it difficult to get any with the stratum adhering to it; " this, however, after several trials I obtained very perfect, and " shall forward to Madras, with specimens of the strata at dif- " ferent depths. The stone is beyond all comparison heavier " than the substance which encrusts it.

"It appears extraordinary how this stone, so concealed, should "under such difficulties have been sought for, and applied to "any purpose; and that the knowledge of the few people who "dig for it, and who do so from father to son, is confined en-"tirely to the finding the stone. For they told me they knew "none of its uses, and that the labour was so hard, and their "gain so small, that they would, through choice, rather work "in the fields; that the sale of it from the spot is confined "solely to the glass sellers, who vend it over the whole coun-"try, and who had, while I was there, above forty Parriar "horses, bullocks, &c. ready in the Pollam, to carry it to Tin-"nevelly, and the southern countries; through which track, "if the stone is known in Europe, I apprehend it has found its "way, by means of the Dutch.

"The people on the spot declare it is to be got in no other "situation or place whatever; and the stone-cutters tell me "they can do nothing without it. It pays no duty, either where "dug up or retailed.

"The colour of the stone is either very light brown or pur-"plish, in the proportion of twenty to one of the latter; but in "use no preference is given, and they are used equally. To an "indifferent person, the most striking circumstance is its great "weight.

"As the spot I have been speaking of now composes a part "of the Company's territories, the most minute information "on the subject may be acquired.

"I felt particular satisfaction in having been the first Euro-"pean who was ever at the place; and I shall be much grati-"fied if the account given meets with your approbation.

"I shall dispatch a load of the stone, in a day or two, which

" I got at the Pollam, with the charge of it. The distance
" from this place, by Namcul, is 84 miles.

The Charges of 50lb. *Weight of Corundum.*

" Nine Tritchinopoly measures of the Corundum stone weigh
" 50lb.

Average and Cost at the Pits.*

	P.	F.	C.
" $1\frac{1}{2}$ Madras fanams per measure - -	—	13	40
" Cooley from thence to Tritchinopoly -	—	28	40
" Ditto from Tritchinopoly - - -	1	13	40
Pagodas -	2	10	40

" The stone is delivered by measures, and paid for at the
" Pollam, in the gold fanam.

" I am, &c.

" EDWARD GARROW.

" Nov. 15, 1792."

This letter contains very interesting topographical observations on the mine. The specimens sent were of one sort, of a greyish colour, with a shade of green. The entire crystals, which I selected among the broken ones, were of course few in proportion; but, with the addition of some distinct crystals, which Col. CATHCART and Capt. COLIN MACAULEY had sent me, have been sufficient to ascertain the structure and form of the crystals, of which an analytical description will close this Paper. I shall, therefore, now say nothing concerning their form, but

* The above is the prime cost. I have been informed by correspondents, who purchased some in retail, that it was sold for about six shillings a pound, at Madras.

proceed to give an account of the varieties of Corundum stone, which I have obtained from India and China.

In the year 1786, Col. CATHCART sent me a small fragment of a stratified mass from Bengal, with this label; "Corundum, "much inferior in price to that of the Coast." It is of a purplish hue; its fracture like compact sand-stone; and a confused crystallization appears in all parts of the stone, by fibres of a whiter colour, from which the light is reflected, as in feld-spar, &c.

I have since obtained a larger lump of the stone, of the same texture, but rather paler in its purplish hue. Sir JOHN MACGREGOR MURRAY informed me that it is called by the natives of Bengal, *Corone*, and used for polishing stones, and for all the purposes of emery.

Its specific gravity is 3,876.

Capt. COLIN MACAULEY procured a lump of Corundum from a *sikuldar*, (a polisher, this term is most appropriate to polishers of steel,) in whose family it had been above twenty years employed, for grinding and polishing stones or gems. The use to which it had been so long devoted had occasioned grooves in its surfaces, which facilitated greatly the examination of its structure. It is about $5\frac{1}{2}$ inches long, $3\frac{1}{2}$ inches broad, and above two inches thick. On one of its broad surfaces are two oval grooves; one of them is four inches long, one broad, and $\frac{3}{4}$ of an inch deep. On the opposite side is a shorter oval groove, above $2\frac{1}{2}$ inches long, $1\frac{1}{2}$ inch broad, and one inch deep. In these grooves, the ends of the laminæ of the mass reflect the light, like the crystals. It serves as a specimen of the simple apparatus of an Indian lapidary. Stones polished in these grooves would be of the common India polish and form,

en cabochon, which is often called tallow drop, from the French lapidaries' term *goutte de suif*, convex, oval, or circular. A very small quantity of the Corundum powder would be required, as the action of the powdered Corundum and gems, on the lump of Corundum, would, as appears from the depth of the grooves, wear away from it a supply of powder, for the operation of polishing. It appears to be part of a larger mass, is of a purplish colour, and of the same laminated texture as the crystals of Corundum; it has this peculiarity, there appear cracks, branching irregularly across the laminæ of the lump, which are filled with homogeneous matter, distinguished however by the superior purity which might be expected to arise from the degree of filtration required for its deposition in the fissures. Some of these cracks, which terminate on the surface, appear to have the same crystallized arrangement which characterizes the laminæ of Corundum. The cracks not being in any degree influenced in their direction by the laminæ of the crystallized mass, it is probable they had not been consolidated, when they cracked; and, from this specimen, we may expect to find Corundum cementing masses of stone, by the same process of stalactitical cementation by which quartz and calcedony connect great nodules and masses of siliceous stones.

In this specimen, I consider the veins as pure Corundum, that is, having the same specific gravity, hardness, and texture as Corundum crystals; and I found the whole lump possessed all the qualities of Corundum, except its specific gravity, which amounted only to 2,785; and in this property it corresponded nearly with the matrix of the Corundum crystals, or the vein in which Corundum is before stated to be found; the specific gravity of which is 2,768. The texture of the matrix

appears sometimes like adularia, and confusedly crystallized; often compact, like cipoline or primitive marble; sometimes sparry, sometimes granulated, and, on the outside of the vein, and near fissures, decomposed, and becoming opaque. In all its states, it scratches glass, but not rock crystal, possibly from want of adherence of its particles; and in this it differs from the substance of the above lump, which cuts glass and rock crystal with great facility.

This lump, and the matrix of Corundum, appeared to possess the same properties as Corundum, when examined by the blow-pipe, with the different fluxes.

The matrix of Corundum having sometimes an appearance like adularia and feldspar, I ascertained, by Mr. HATCHETT'S scales, the specific gravity of adularia to be 2,558, and of feld-spar 2,555. The Corundum, and the lighter Corundum of the lump, cut adularia and feldspar; the latter effervesced, and combined with soda, which the former did not.

It is therefore evident, that the matrix of Corundum, or substance of the vein, is a distinct substance from adularia and feldspar, and nearly connected with Corundum.

The matrix or vein contains also a black substance, like shorl, which, on closer observation, appears to be *hornblende.* This substance Mr. GARROW had remarked to have the appearance of charcoal, and, on that account, he had attributed the formation of these strata to the agency of fire. Other gentlemen, from the appearance of the matrix of Corundum, have stated it to be a calcareous vein.

Mr. GARROW observed, that there ran through the strata in which the Corundum was found, veins of a substance like dried pitch, apparently on their edge, which separated like a pack of

cards. It is a brown micaceous substance, which in drying foliates, and shews a certain degree of regular arrangement of the component parts; in this case, the fragments of the folia subdivide, with some degree of regularity, into rhombs, whose angles are 60° and 120°: it is more smooth, and less flexible, than pure mica.

These are all the sorts of Corundum which I procured from India.

I now proceed to the result of my inquiries in China.

I requested Capt. CUMMING, in 1786, at that time commanding the Company's ship Britannia, to take a specimen of Corundum to China, to ascertain its nature, and to obtain specimens, if possible, adhering to their matrix, and regularly crystallized. On his arrival at Canton, he collected the information I wished, with the good sense and zealous desire which he always exerts for his friends. He ascertained that the stone I inquired for, was in common use with the stone-cutters; and he brought me the stone, in its rude and in its pounded state, taking care to select the most regularly crystallized pieces, and others adhering to the rock. A stone-cutter was sawing rock crystal with a hand-saw, which he also brought to me; it is a piece of bamboo, slit, about 3 feet long, and 1¾ inch broad, thickened at the handle by a piece of wood, rivetted with two iron pins; having a lump of lead tied with a thong of split rattan, steadying an iron pin, on which the end of a twisted iron wire is fastened, which, being stretched to the handle, is passed through a hole in the bamboo, with the superabundant wire; a wooden peg, being pressed into the hole, keeps the bow bent, and the wire stretched, and serves to coil the superfluous

wire, till, by sawing the crystal, the stretched wire is worn, and requires to be renewed from the coil. The twisted wire answers the purpose of a saw, and retains the powder of Corundum and water, which are used in this operation. Dr. LIND had before brought specimens similar to the above, from China.

From Sir JOSEPH BANKS I obtained Dr. LIND's specimens, and some in powder, which Mr. DUNCAN, supercargo in China, had sent him, with the Chinese name, *Pou-sa.* The matrix, being mixed with a red and white sparry substance and mica, is generally called red granite; but it appears to me of the same nature as the matrix of Corundum from India. The white is more fibrous, and like cyanite; the red part of it is compact and opaque; other parts appear to foliate, and pure mica is in considerable patches, and generally adheres to the crystals. This Corundum is of a darker brown, and more irregular on the surface than the Corundum of the Coast, and often mixed with black iron ore,* attractable by the magnet. It is described as the third modification of the Corundum crystal, in the analytical description which follows. The *chatoyant* or play of light, on these dark crystals, is very remarkable; some are of a bright copper colour; others exhibit the accident of reflection of light, which, in a polished state, gives varieties to the cat's eye, star-stone, sun-stone, *&c.*; which, as yet, are classed from such accident, without strict attention to their nature, which is various, and in general has not been ascertained.

* A small group, consisting of three or four octoedral crystals, presents the least common variety of this kind of iron ore; the edges of the octoedra being replaced by planes which almost cover the triangular planes. *Romé de l'Isle. Cristallog.* Vol. IV. Plate 4. fig. 69.

These are the circumstances connected with the strata worth mentioning. The examination of Corundum on which our present knowledge rests, is nearly that which an Indian mineralogist might derive of the history of feldspar, from a lump of Aberdeen granite, out of one or two different quarries. He might ascertain a few modifications of the crystal of feldspar, its fracture, and matrix; but he would have no knowledge of the purest or more beautiful sorts which other quarries produce, in Scotland, at Baveno, at St. Gothard, and in Auvergne. I therefore think it essential to mention, that Corundum, under circumstances favourable to its crystallization, becomes glassy in its fracture, and of various colours. I have not only observed, in crystals of Corundum, specks of a fine ruby colour, but I have fragments of crystals, in texture and every respect like the colourless Corundum, of a fine red colour. It is certain that we obtain from India, Corundum which may pass for rubies. I have sent to India some of the Corundum with small ruby specks, which were not sufficiently distinct or large either for measurement or analysis, in hopes of being enabled to ascertain correctly the form of *Salam* rubies found in Corundum; in the mean time, I have the Corundum of a fine red colour. Looking over some polished rubies from India, I selected one which appeared laminated like Corundum, and had also the *chatoyant* or play of light on its laminæ, which formed an angle in the stone. The lapidary called it an Oriental ruby. I altered the form of the cutting, so fortunately, that the reflected rays formed a perfect star; a phænomenon I had observed in the sapphire, and expected in Corundum, but not in the octoedral ruby. The specific gravity of

this stone, being 4,166, confirmed my opinion that it is one of the *Salam* rubies, so much esteemed by the natives on the Coast or Peninsula of India, which are found in the Corundum vein. The specific gravity of a colourless sapphire, very little less opaque than Corundum, forming also a perfect star, was 4,000: that of a deep blue sapphire, and of a star-stone, 4,035; all which I connect with the Corundum; the specific gravity of a distinct crystal of which was 3,950; of a fragment of ruby-coloured Corundum, 3,959; and of a fragment of Corundum with vitreous lustre, 3,954.

It may be objected to me, that BERGMAN has stated the variety of specific gravity in gems to be so great, as to leave no certain rule of judging thereby of the species. He observed, that the topaz generally prevails in weight, being from 3,460 to 4,560; the ruby from 3,180 to 4,240; then the sapphire, from 3,650 to 3,940.* But in the preceding page he had said, "Analysi crystallorum, tam ejusdem quam diversæ figuræ, multum lucis scientia expectat. Illæ quarum antea compositionem explorare licuit, naturali forma per artem privatæ erant." It is not, therefore, an hypothesis unworthy of examination which I advance, that gems derived from the rectangled octoedra, whose specific gravity is above 3,300 to 3,800, will be found to be diamonds or octoedral rubies; and these will be easily distinguished from each other, by their lustre and hardness. Diamonds, whether red, yellow, blue, or white, being hardest, though their specific gravity will be less; *viz.* from 3,356 to 3,471, as I found among different diamonds in my collec-

* *De Terræ Gemmarum. Bergm. Opusc.* Vol. II. p. 104.

tion: whereas the octoedral ruby was from 3,571 to 3,625, and inferior in hardness, not only to the diamond, but to the Corundum; the specific gravity of which, in its different appearance of form and colour, I found to vary from 3,876 to 4,166; and I suppose it to be subject to a variation from 3,300 to 4,300: after which, the jargon will come, with a specific gravity of 4,600; easily distinguished also, by its crystallization, from the abovementioned gems. The above specific gravities, Mr. HATCHETT very obligingly assisted me in taking, with his accurate scales, in the temperature of 60°. It will not be understood that I depend entirely on the specific gravity; on the contrary, I connect this quality with crystallization: hardness is the next criterion; and analysis must separate the component parts, and demonstrate the analogy or identity of substances, or of compounds. The improvements of Mr. KLAPROTH's process are evident, by the comparison of his first analysis, and his last analysis, of Corundum.

In the first it consisted of

Corundum earth	-	68 0
Siliceous earth	-	31 50
Iron and nickel	-	0 50
		100

By the last analysis of Mr. KLAPROTH, the Corundum of the Peninsula of India consisted of

Argillaceous earth	-	89 50
Siliceous earth	- -	5 50
Oxide of iron	- -	1 25
Loss	- - -	3 75
		100

The Corundum of China.

Argillaceous earth	-	84 0
Siliceous earth -	-	6 50
Oxide of iron -	-	7 50
Loss - -	-	2 0
		100

That the analysis of sapphire by Mr. KLAPROTH may be compared, it is here added.

Argillaceous earth	-	98 50
Calx of iron - -	-	1 0
Calcareous earth	-	0 50
		100

Iron ore crystallized is often mixed with the Chinese Corundum, as I have before stated, and may be considered as accidentally interposed, not combined. In the Corundum of the Coast, the greenish colour may indicate the combination of iron, as the blue colour does in the sapphire; and the proportion of iron in both is nearly alike.

There then is the $\frac{5\ 50}{100}$ and $\frac{6\ 50}{100}$ of silex in Corundum, evidently an integral part of the coarse Corundum crystal, and not of the sapphire; but it will require an analysis of the vitreous or pellucid Corundum, to decide that silex is a constituent part of Corundum: there will then remain to account for the calcareous earth; and, having established its being a constituent part of the sapphire, the small proportion of $\frac{0\ 5}{100}$, cannot be expected to produce a very notable difference.

It is not necessary to do more than thus to hint at what further analysis and examination of former experiments are

required, to ascertain the analogy or identity of the sapphire and Oriental ruby with Corundum.

I have before stated, that I have Corundum (which has the same texture and fracture as the common colourless Corundum) of a ruby red, and also of sapphire blue, and of sapphire blue and white colours.

I have sapphires, yellow and blue, white and blue, brown and greenish, and of a purplish hue; these I should consider as Corundum, with fracture of vitreous lustre.

Mr. TRANCKELL, who resides in Ceylon, and from whose communications I derived lately much information, had, about five years ago, a sapphire, the greater part blue, and the remainder of a pale ruby colour. I saw, in ROMÉ DE L'ISLE's collection, at Paris, a small gem, which was yellow, blue, and red, in distinct spots, and he called it Oriental ruby. M. DE LA METHERIE, to avoid the confusion of the denomination Oriental ruby with octoedral ruby, calls it a sapphire; with more correctness, I think, the abovementioned gems should be classed as argillaceous, under the denomination of Corundum.

I am not uninformed that Corundum is said to be found in France. The Count de BOURNON is convinced, that the specimens mentioned in CRELL's Journal, as having been found by him in a granite in the Forez, were Corundum. M. MORVEAU also says, he found it in Bretagne; but the Abbé HAUY, in No. 28 of the *Journal des Mines*, asserts, that the Corundum found in France is titanite: he does not say whether this observation extends both to the Corundum of Bretagne and that of the Forez.

In the same manner I had observed, in the specimens which Mr. RASPE called Jade, or a new substance, from Tiree,

on the west coast of Scotland, a great resemblance to Corundum; but, having then only had a cursory view of the substance, I am indebted to Mr. HATCHETT for the examination of a specimen of it which he had from Mr. RASPE's collection.

The Tiree stone resembles crystallized Corundum of the Coast, in texture and colour; it is also as refractory, when examined by the blow-pipe, with different fluxes. Its specific gravity is 3,049; consequently nearer the specific gravity of pure Corundum than the abovementioned lump, 2,785, and the matrix of Corundum, 2,768. The Tiree stone will scratch glass readily, but not rock crystal; its hardness therefore corresponds with that of the matrix of Corundum. The substance of the lump described in page 410, cuts glass, and rock crystal, and the Tiree stone, readily.

It will therefore be sufficient for me to say, that there is great probability Corundum may be found in Great Britain, and on the continent of Europe, as well as in Asia; and the above slight assays may show, that observations on Corundum, in its different states of purity, may lead to accurate distinction between substances hitherto imperfectly known, and will lead to a revision of the siliceous genus, whereby the argillaceous genus may obtain its due pre-eminence in mineralogy.

When gems, by art, or by rolling in the beds of rivers, have been deprived of the angles of their crystals, they are unavoidably subjected to uncertain external characters, which even great practice cannot render certain; and hence the unwillingness of European jewellers to deal in coloured gems. I have some specimens of a sapphire-blue stone, India cut, very small and pellucid; they were purchased in India, as sapphires, and were supposed to be fluor by a lapidary in London, but are

cyanite. The above could scarcely have happened, if the stones had been of sufficient size and value to require much examination, the weight and degree of hardness being exceedingly deficient. The colour, therefore, will not be a safe guide. The diamond, whether white, blue, red, yellow, or green, can be distinguished by its crystal, or by its specific gravity and hardness, or, when it is polished, by its lustre. Other stones which compose the order of gems, might equally depend on their crystallization, specific gravity, polish, and hardness, for a distinct arrangement. The near relation of argil, which BERGMAN gave to this order, is daily confirmed; and it will perhaps be to Mr. KLAPROTH, more than to any other existing chemist, that we shall owe our correct information on the subject of other gems, as we do on the subject of Corundum.

Many of the varieties of Corundum, particularly the coloured and transparent sorts, with their regular crystallizations, are yet *desiderata*. Many crystallized stones, from defect of colour, lustre, *&c.* are of little value in the market, such as, jargon, chrysolite, tourmaline; and an infinity of unnamed stones of Ceylon, Pegu, Siam, *&c.* would be valuable to the mineralogist, if obtained adhering to their strata, and in crystals whose external form is not obliterated. I have no doubt, when it is known how much such information will tend to illustrate the history of the earth, and particularly that of gems, the spirit of inquiry, so laudably afloat in British India, will be directed to attain it.

I have not heard of any metallic veins being found in Corundum, unless a stone which ALONSO BARBA, lib. i. c. 13. describes, should give an instance. "The Chumpi, so called "from its grey colour, is a stone of the nature of emery, and

"contains iron; it is of a dull lustre, difficult to work, because "it resists fire long. It is found at Potosi, at Chocaya, and "other places, with the minerals negrillos and rosicleres."

Having mentioned the varieties of crystallized and amorphous Corundum, and the miscellaneous facts relative to my collection of that substance from India and China, it might be sufficient to give an *icon* of the crystal, and close a paper already prolix; but, having with satisfaction observed, within the last years, the science of mineralogy gaining ground in Great Britain, from the knowledge acquired by several gentlemen who have examined the mines, and formed personal acquaintance with the most experienced and learned men on the Continent, and also from ingenious foreigners, who have communicated their observations on English fossils, and connected them with the most approved systems, it may perhaps be accepted as a sufficient apology for what follows, that I consider it as a *desideratum* to English mineralogists, to be invited to a preference of permanent characters, which the study of crystallization has collected, and which promises to be a certain method of ascertaining the laws by which elective attraction arranges and combines molecules of matter.

It is true, the progress of crystallography has been extremely slow, and different nations have contributed to its present improvement. It is rather remarkable, that the earliest treatise on metallurgy, of authority, was published in Italy, by VANOCCIUS BIRINGUCCIUS, just before AGRICOLA published his treatise in 1546 in Germany; and the first treatise on the structure of crystals I know, is also from Italy, by NICOLAS STENO, *Prodromus Dissertationis de Solido intra Solidum naturaliter contento. Florentiæ*, 1669, in 4to. A work of great

merit. LOUIS BOURGUET of Neufchatel, in his *Lettres sur la Formation des Sels et des Crystaux. Amst.* 1729, 12°, connected, by observation and measure, triangular and rhomboidal, and cubic and pyramidal tetraedal molecules, for all different substances. His contemporary, MAURICE ANTOINE CAPELLER,* attempted to deduce a system from geometrical principles; and in this state did LINNÆUS find the subject, when he attempted to reduce the science of minerals to external characters, and crystallized bodies to salts.

None of the observations of LINNÆUS will prove useless to science; but his system alarmed the chemists and mineralogists, who rejected every other criterion than internal character from analysis, and the system of CRONSTEDT was preferred by general assent. By this means, a spirit of controversy deprived the chemist and lythologist of mutual assistance; and the general opinion was correct, on the supposition that a mixed system of chemical and external characters would be irreconcileable; but it has been admitted, even by those who most decidedly opposed LINNÆUS's system, that the best system of mineralogy should be founded on external and internal characters combined.† Among the few who ventured to profess their obligations, at the same time, to LINNÆUS and to CRONSTEDT, was Baron BORN, whose abilities and character, in addition to his distinction as one of the counsellors of mines

* *Prodromus Crystallographiæ,* &c. and *Litteræ ad* SCHEUZERUM, *de Crystallorum Generatione. Act. Nat. Cur.* Vol. IV. *Append.* p. 9.

† Nullum itaque est dubium, quin hujusmodi methodus mixta, quæ notis characteristicis tam extrinsecis quam intrinsecis simul combinatis, est superstructa, proxime ad naturalem accedens, maximam indicans symmetriam, reliquis sit præferenda methodis. *J. G.* WALLERIUS, *de Systemate Mineralogico rite condendo,* §. 102.

of his Imperial Majesty, obtained his enrollment among the Fellows of the Royal Society. He connected the intrinsic and extrinsic characters of minerals, in the *Index Fossilium*, which he published in 1772. In Sweden, Bergman's treatise on the forms of crystals, published in the Upsal Transactions, in 1773, was a more authoritative recommendation to the investigation of the principles of crystallization; and it can be of little importance for me to add, that since I have possessed the collection of Baron Born, in 1773, I have had every confirmation of the same opinion. The progress of chemistry and of crystallography, applied to mineralogy, has rendered the examination of strata, and of mines, a source of amusement as well as instruction; and the arrangement of interesting facts, in the chemistry and mechanism of nature, suits my occasional researches in geology, which, from variety of avocations and circumstances, have been very much interrupted. My acknowledgment of obligation to the learned who have made this progress in science, is the best recommendation I can give to others to examine their works. Those whose talents and time are devoted to the investigation of every mineral substance, can have no respite to their labour; minerals, in every state of their formation, perfection, and decomposition, as they occur in mines, must have their qualities immediately ascertained, and be reserved for profit, or thrown away on the heap. The practical miner could not, without external characters, make any progress. The valuable minerals are soon pointed out by assay, and their appearance remembered. The accuracy of selection depended, in all periods, much on the experience of the miners. It remained for Mr. Werner to give the utmost degree of accuracy which irregular external characters can acquire, by fixing

appropriate terms to all the characters which occur, and which the senses can discriminate. In 1774, he opened his system of external characters of minerals, and the perfection he has since given to it, has rendered it very general. The LESKEAN collection, arranged after Mr. WERNER's method, has procured, in Mr. KIRWAN, a powerful support to the introduction of that system in this country; and we have already some other valuable publications, to recommend and introduce other favourite systems of the Continent. It is, therefore, at this time the English mineralogist should be invited to examine, if not to prefer, permanent characters, so far as the progress of crystallography has collected them, or at least to give them a distinguished rank among external characters of bodies.

If prejudice too long has retarded the union of intrinsic and extrinsic characters, it has also occasioned a schism among the advocates of crystallography.

ROMÉ DE L'ISLE, in the year 1772, published the first edition of his Essay on Crystallography, which he states to be a supplement to LINNÆUS; and, by the assistance of a very few friends, he was enabled to increase the number of crystals in a degree to assume the appearance of a system. He told me, that the accuracy of his measurement of angles of minute crystals was the acquirement of great practice, but that the Count de BOURNON, after a short practice, attained equal correctness, and afforded him assistance, which he acknowledges in his 2d edition to have received, particularly by the discovery of crystals in Dauphiné, Auvergne, Franche-Comté, *&c.*

The Abbé HAUY, an accurate and patient observer, and a good mathematician, considered crystallography as founded on certain laws, reducible to demonstration by calculation. In the

beginning, the differences of BOURGUET and CAPELLER were not more pointed than those of ROMÉ DE L'ISLE and the Abbé HAUY; but the progress of observation and calculation having demonstrated their mutual utility, the observer and measurer of crystals will now rest satisfied only when calculation confirms actual measurement. To the Abbé HAUY is also due a late scheme to simplify calculation, by expressing, according to algebraical formulæ, the different laws which determine the modifications of crystals. So far as they are the result of calculation and measurement, we may admit the laws of crystallization; for, whenever the superposition, or subtraction, of simple or compound molecules on a nucleus, shall, by calculation, give a series of planes and angles, which corresponds exactly to the angles and planes measured on natural crystals, it will amount to no more nor less than a demonstration of the rule or arrangement of elective attraction by figures.

These laws may be reduced to simple practice; for instance, the Abbé HAUY, by measuring the rhombic plane of Corundum, found its two diagonals to be as two to three; which gives to its acute angle 81° 47′ 10″, and to its obtuse angle 98° 12′ 50″; the same as martial vitriol.*

The forms of fragments in Corundum are all acute rhomboids. The cosine of the little angle in Corundum is $\frac{1}{7}$ of the radius; but, in calcareous spar, the cosine is $\frac{1}{5}$ of the radius; in shorl, $\frac{2}{5}$ of the radius; in the garnet, $\frac{1}{3}$; and, in rock crystal, $\frac{1}{17}$.

Thus, the application of general laws, to ascertain constant

* This result is extracted from the *Journal de Physique*; but it appears, from the *Journal des Mines*, No. 28, that the Abbé HAUY has since rectified this measure, and given 86° 26′ for the acute angle, and 93° 34′ for the obtuse angle.

character, after they shall have been fully verified, may be very simple and general. It will not require perfect crystals; for, when crystals separate into laminæ, which subdivide into fragments, and shew the form or arrangement of their molecules. it is easy, from such fragments, to connect them with their primitive crystal, and consequently with their class. It will be a great step, to obtain one regular and permanent external character. Attention to other characters will be necessary, to ascertain the nature of the substance; and other external characters, such as irregular fracture, colour, &c. must be resorted to, where no permanent characters exist; but from their nature they are fallible, and in fact are seldom conclusive.

The progress of crystallography appearing to me of consequence to the progress of mineralogy, induced me to desire the Count de BOURNON, abovementioned, one of the honourable victims to his allegiance to his King, to describe such crystals, in my collection, as shewed the different known modifications of Corundum; which will develop the theory of crystallization, so far as is consistent with the avowed object of this Paper. The subject, I believe, has not hitherto been submitted to the consideration of this Society. The translation of the Count de BOURNON's description has been carefully made to preserve its clearness, and I hope it will be favourably received by the Society, and make some amends for my tedious introduction.

After it, I have added a table, connecting in one view the specific gravities of Corundum, *&c.* herein mentioned, with those given by other authors.

An Analytical Description of the Crystalline Forms of Corundum, from the East Indies, and from China. By the Count de Bournon.

The most usual form of Corundum is a regular hexaedral prism; (Tab. XXII. fig. 1.) in general, the surface of the crystal is rough, with little lustre, owing to unfavourable circumstances under which it crystallized.

The crystals of Corundum hitherto found were not formed in cavities, where, each crystal being insulated, its surface could preserve that smoothness and natural brilliancy which are common to all substances that freely assume a crystalline form. Like the crystals of feldspar which we meet with in the porphyroid granites, the Corundum crystals have been enveloped, at the time of their crystallization, by the substance of the rock which was forming, at the same time with themselves, in an imperfect and confused crystalline mass; and the Corundum crystal, before it had acquired its perfect solidity, necessarily received on its surface the impression of the different particles of the rock which enveloped them: this naturally renders the surface rough and dull. Crystals of feldspar found in the granitic porphyroid rocks, exhibit the same kind of appearance, from the same cause.

The Corundum crystals are in general opaque, or at least they have only an imperfect transparency at the edges: when broken into thin fragments, the pieces are semi-transparent: when held between the eye and the light, and examined with a powerful lens, it will be perceived that their interior texture is rendered dull by an infinite number of small flaws crossing

each other, much resembling the medullary part of wood, when it is viewed in the same manner.

The degree of transparency of the small interstices which are between these flaws, is further evidence that this texture of small flaws occasions opacity, which augments in proportion to the thickness of the fragments. This kind of internal structure has also a very strong analogy with that of feldspar in granite and porphyry.

The endeavour to split these crystals, in a direction either perpendicular or parallel to their axes, meets with a very considerable resistance: they may indeed be broken in these directions; but the rugged and irregular surface of the broken parts, clearly proves that the direction in which the crystalline laminæ have been deposited one upon another, has not been followed.

The regular hexaedral prism of these crystals, cannot therefore be considered as the form of the nucleus of the crystal; and consequently is not the primitive form of the crystals of this substance.

If, in order to discover the direction of the crystalline laminæ, a variety of crystals be examined, some will hardly fail to be met with, which, on their solid angles, formed by the junction of the sides of the prism with the planes of the extremities, present small isosceles triangles. These are sometimes greater and sometimes smaller, and form solid angles of 122° 34′, with the extreme planes of the crystal. They are in some instances real faces of the crystal; but most frequently they evidently are the effect of some violence on that part. The smoothness and brilliancy of these small faces, in the latter case, shew that a piece has been detached in the natural direction of the crys-

talline laminæ. It is indeed much less difficult to separate a portion of the crystal at these angles, than at any other part; and, in following the natural direction of the faces, with a little patience and dexterity, all the crystalline laminæ may be detached, and progressively increase the size of the triangular face.

This operation, however, cannot be done indiscriminately on all the solid angles of the crystals, but only on the alternate ones at the same extremity, and in a contrary direction to each other. As to the other angles, they may be broken, but it is impossible to detach them. When, instead of the solid angles of a hexaedral prism, small triangular planes are met with, (which frequently happens, whether caused by violence or otherwise,) they are always placed in the direction above-mentioned.

If, by following this indication of nature, we continue to detach the crystalline laminæ, we shall at last cause the form of the hexaedral prism to disappear totally, and, in place of it, a rhomboidal parallelopiped will be obtained, (fig. 2.) of which the plane angles at the rhombs will be 86° and 94°; the solid angles at the summit* will measure 84° 31′; and that taken at the re-union of the bases will be 95° 29′.

We can split this parallelopiped only in a direction parallel to its faces; it will still consequently preserve the same form, which is that of the nucleus of this substance, and its primitive form.

It is, therefore, by a modification of the rhomboidal parallelo-

* For greater clearness, this rhomboidal parallelopiped may be considered as being formed by the junction of two triedral pyramids, base to base; and the two solid angles (each of which is formed by the re-union of three of the acute angles on the planes of the rhomb) will then be considered as the summits of these pyramids.

piped, (fig. 2.) that nature has formed the regular hexaedral prism (fig. 1.) which this substance presents.

For, if we conceive, that in any period whatever of the increase of the rhomboidal parallelopiped, a series of laminæ or crystalline plates has been deposited on all the sides of the parallelopiped; and that these laminæ have all undergone a progressive decrease of one row of crystalline molecules, at the acute angle which tends to form the summit, and also along the sides of the opposite acute angle, (fig. 3. and 4.) there will necessarily result from the continuation of this superposition, to a certain period, an hexaedral prism, terminated by two triedral pyramids, placed in a contrary direction; and their planes or faces, which form a solid angle of 147° 26′, with the sides of the prism, will be either pentagonal, (fig. 3.) or triangular. (Fig. 4.) They will also have, in place of a summit, an equilateral triangular plane, sometimes greater and sometimes smaller.

If the superposition continues, the equilateral triangular plane on the summit will become nonagonal, and there will remain no other traces of the primitive planes of the rhomboidal parallelopiped, than small isosceles triangular planes: (fig. 5.) if the superposition still continues, until the last crystalline lamina is reduced to a single molecule or point, no appearance of the rhomboidal parallelopiped will then remain; and the crystal resulting from this operation of nature will be a regular hexaedral prism. (Fig. 1.)

In the same manner, *viz.* by a decrease on the lower edges of the laminæ, the primitive rhomboidal parallelopiped of calcareous spar passes to a regular hexaedral prism of that substance; though more frequently it does so by a decrease on the lower angles of the laminæ.

When the laminæ of the Corundum crystal have, during their superposition on the planes of the primitive rhomboidal parallelopiped, experienced a progressive decrease at one of their acute angles, and along the sides of the other, at the same time, and in the same proportion, it is easy to conceive that the height of the hexaedral prism must be the same as that of the rhomboidal parallelopiped, upon which it has been formed. The height BC (fig. 1.) must therefore bear the same proportion to the line AB, drawn through the middle of the two opposite sides of the planes on the extremities, as the whole height EF, of the rhomboidal parallelopiped, (fig. 2.) bears to the small diagonal GH, from one of the rhombs; that is, nearly as 6,45 : 5.

But, although this exact proportion appears in a very great number of Corundum crystals, yet we meet with some whose lengths are more or less considerable; and this is owing to different circumstances which have existed at the time of their crystallization. We may conceive, for instance, that if, before the progressive decrease of the crystalline laminæ, in the manner abovementioned, the increase of the rhomboidal parallelopiped had taken place by a superposition of laminæ, in which the rows of crystalline molecules experienced a progressive decrease along the edges of the acute angle of the base only, (fig. 6.) and that (the sides of the prism having already acquired a certain length) the succeeding crystalline laminæ had experienced a decrease at the acute angle of the summit, the same regular hexaedral prism would have resulted from this process; but the proportion between the height and the line drawn from two of the opposite sides of the planes on the extremities, would have been much greater than that of

6,45 : 5; and, consequently, this prism would have been longer than that of the rhomboidal parallelopiped which served as its nucleus.

On the other hand, if the increase of the rhomboidal parallelopiped had taken place by a superposition of crystalline laminæ, decreasing at the acute angle of the summit, and, some time after, decreasing also along the sides of the acute angle of the base, (fig. 7.) the regular hexaedral prism resulting from this process would have been shorter, in proportion to the duration of the mode of decrease in the crystalline laminæ which were first deposited. There are some of the hexaedral prisms, in Corundum crystals, which are so short, that they appear no more than segments. Calcareous spar offers the same phænomenon; as do likewise all the substances in which the hexaedral prism has any analogy of formation with that which we have here described.

It happens frequently, when the superposition of the crystalline laminæ does not go on equally on all the faces of the rhomboidal parallelopiped, that one or two only of the solid angles of the hexaedral prism, taken alternately, still shew, by small isosceles triangular planes, some remains of the faces of the parallelopiped, while the others do not shew any at all.

Mr. Greville, in his collection of this substance, has a crystal of Corundum, upon one side of which, only two of the planes of the rhomb have experienced an equal and perfect superposition, while there has been but a very small number of crystalline laminæ deposited on the third plane. Consequently, this crystal presents a regular hexaedral prism, one of whose solid angles is so much truncated, that the half of the plane of the end of the hexaedral prism disappears; (fig. 8.)

and this cut or section forms an angle of 122° 34′, with the plane on the extremity.

It is unnecessary to observe, that the regularity of the hexaedral prism, depends on that of the rhomboidal parallelopiped on which it is formed.

When, by detaching the laminæ from the alternate solid angles of the regular hexaedral prism, the planes resulting from this operation begin to run into one another, and the crystal begins to assume the form of the rhomboidal parallelopiped to which it owes its origin, we frequently see the surface of these new planes divided into an immense number of small rhombs, formed thereon by the intersection of lines that are parallel to the sides, which belong to the rhomboidal form of the new faces. (Fig. 9.)

These lines are owing to the extremities of the laminæ which have been deposited on the inferior faces, corresponding with those on which we observe them; and they serve to corroborate still farther, the demonstration we have given of the formation of the regular hexaedral prism in this substance.

We frequently see small rhombs traced on the surface of the planes, on the ends of the hexaedral prism. (Fig. 10.) This, no doubt, is occasioned also by the intersection of the laminæ, on the planes of the primitive rhomboidal parallelopiped. But these rhombs, formed by the re-union of lines that join in angles of 60° and 120°, instead of 86° and 94°, (like those we have seen traced on the faces which correspond with those of the rhomboidal parallelopiped,) form angles of 60° and 120°. It would therefore be an error to consider them as indications of the form of the elements of crystallization, as we are tempted to do from a simple inspection of the crystal. These same lines

form equilateral triangles with one another, as may be seen in fig. 10.

The cause of these small equilateral triangles, which sometimes project a little over the planes on the ends of the prism, must now be obvious. If, during the superposition of the crystalline laminæ on all the planes of the rhomboidal parallelopiped, it has happened, from any cause whatever, that the laminæ deposited on the three faces of the same summit, have not fallen exactly on those which preceded them, or that they have experienced some deviation, or have not had the same decrease as all the others, at the angle of 86°, these triangles must necessarily occur; in the same manner it must be obvious, why these small equilateral triangular projections are frequently placed on one of the sides of the crystal.

The primitive form of the Corundum crystal is therefore a rhomboidal parallelopiped, whose solid angle at the summit is 84° 31′, and that formed by the re-union of the bases is 95° 29′.

The crystalline laminæ are rhombs of 86° and 94°: these, in my opinion, are double crystalline molecules; the single molecules I apprehend to be isosceles triangles, of 86° at the angle of the summit, and of 47° at those of the base.*

* I am at present preparing a work, in which I shall, if circumstances permit me to finish it, give the result of my observations, and my own opinion on this interesting part of mineralogy. I shall only observe here, that although double molecules, square and rhomboidal, are frequently formed in the process of crystallization, yet the real form of the crystalline molecules seems to be triangular. By observing the progress of the rhomboidal parallelopiped, in its passage to the form of an hexaedral prism, (fig. 4. and 5.) and by considering the prism terminated, it seems evident, that the last lamina which had been deposited, after the progressive decrease in the rows of crystalline molecules to one single molecule, must necessarily have been triangular.

Although the rhomboidal parallelopiped of 86° and 94° is the primitive form of the Corundum crystal, yet it is rare to meet with that substance under this perfect and determined form; and, in most mineral substances, it is more rare to meet with their primitive crystals than their different modifications. Amongst Mr. GREVILLE's numerous specimens of Corundum, I have met with only one which has this primitive form, and it is doubtful whether even this may not be a fragment.

The Corundum crystal presents another modification, under which the regular hexaedral prism, instead of having three alternate solid angles at each of its ends, (on which solid angles are placed isosceles triangular planes, forming a solid angle of 122° 34′, with the planes at the extremities upon which they are inclined,) has also its angles supplied by isosceles triangular planes; but these planes, instead of 122° 34′, form solid angles of 160° 42′, with the said planes on the extremities. (See fig. 11. and 12.) These new planes, which constitute a new modification of the primitive form of Corundum, are the result of a different order in the decrease of the laminæ; which, in the primitive form, are deposited on the planes of its primitive rhomboid by single rows of crystalline molecules, and increase the planes which terminate the hexaedron: whereas, in this second modification, the decrease of molecules is by two rows, which gives a more obtuse inclination, and forms new planes. The surface is usually striated, parallel to the sides of the planes which terminate this crystal; an appearance always announcing imperfection in the crystallization, arising either from a change in the order of decrease or increase, or from a less perfect union of the crystalline laminæ. A section would show gradual risings or steps, as appears in fig. 14. which is a section

of fig. 13. in the line ADB. These striæ are not to be confounded with those in numberless substances, as in tourmalines, schorls, *&c.* which arise from the longitudinal union of numberless distinct crystals. The crystal resulting from this new mode of decrease in the crystalline laminæ, will represent one or other of the varieties shewn in fig. 11, 12, and 13, according to the period when such decrease has begun in the process of the crystallization; and, if it has begun very late, the new faces will only be small, nay almost imperceptible, isosceles triangles, forming solid angles of 160° 42′, with the planes of the extremities of the prism, as in fig. 5.; the measure of the angles however must be excepted.

If this irregular mode of decrease had begun with the first crystalline laminæ which were deposited on the primitive rhomboidal parallelopiped, the hexaedral prism resulting therefrom would have been terminated by two very obtuse triedral pyramids, whose planes would have been rhombs; and they would have been placed in a contrary direction to each other, as may be seen in fig. 12, by the dotted lines. I have not met with this variety, but its existence may be supposed.

It happens sometimes, that the crystallization has not been so perfect as to destroy every appearance of the faces of the primitive rhomboidal parallelopiped; in this case, there remains, on the solid angle of 112°, formed by the junction of the new faces with the edges of the prism, a small isosceles triangle, as in fig. 13, which corresponds to those in fig. 5. of the preceding modification.

The crystals which explained the second modification, form also a part of Mr. GREVILLE's collection: one, in particular, is highly worthy of notice; it is the most perfect crystal I have

ever seen of this substance. The surface of the faces of the prism, although rough, is infinitely less so than that of the others, and much more brilliant. The planes on the ends have the usual polish of crystals; its colour is a pale red, and its transparency may be compared to that of wax.

This substance presents a third modification, in which the hexaedral prism diminishes in diameter, as is apparent by comparing the diameters of its two ends; in some, it appears like a regular hexaedral pyramid truncated. (Fig. 15.) The crystals of this modification are usually irregular, and seldom admit of a certain measure of their angles; but, among the numerous specimens in Mr. GREVILLE's collection, I have been able to ascertain, in the greater part, that the hexagonal plane at the top forms angles of about 120°, with the planes of the pyramid; and the hexagonal plane at the base forms angles of about 78°, with the planes of the pyramid. In other instances, the form of the pyramid varies greatly; in some, the angle at the upper plane was 110°, and the angle at the base about 70°; in others, the angle at the upper plane was about 100°, and the one at the lower plane about 80°.

In these three varieties, the crystalline laminæ can be separated, as in the hexagonal prism, at the three solid alternate angles of each end, but in a contrary direction to each other. The planes which appear when the laminæ are detached regularly, form solid angles of 22° 34′, with the planes of the extremity: this arrangement is analogous to that of the hexaedral prism. The difference of form arises from the crystalline laminæ deposited on the planes of the primitive rhomboid, decreasing by more than one row of molecules, on the planes of one of the triedral pyramids of the rhomboid, and by less

than one row, on the planes of its other pyramid. This general observation, on the manner in which this primitive crystal of Corundum passes to the different varieties just mentioned, is the only one I have established with any great degree of certainty at present. Specimens with perfect crystals, whose angles may be measured with accuracy, will probably arrive from India, and give further demonstration, as to these and other varieties of modifications of Corundum. We may conceive, that if, in this modification, the crystallization had ceased before the entire formation of the crystal, there would have remained small isosceles triangular planes, on three of the alternate solid angles, formed by the junction of the planes on the ends, with the edges of the truncated pyramid. These isosceles triangular planes resemble those we have seen in the first modification; (fig. 4. and 5.) and form, in the same manner, solid angles of 122° 34′, with the planes on the ends of the prism. (Fig. 16.)

Finally, if, during the formation of the crystal, in this modification, it should happen that the laminæ deposited on the three planes of the rhomboidal parallelopiped, on the side where they undergo a greater decrease, do not undergo the decrease of one row of molecules at the acute angle of the summit, the crystal will be a real hexaedral pyramid, (fig. 17.) whose acute angle at the summit, measured on the sides, will be nearly 24°, in one of the varieties; 40° for the most obtuse; and 20° for the most acute variety: the angle of their triangular planes, in the first instance, 13° 41′; in the second, 22° 20′; and 11° 28′ in the third. I have not seen any perfect pyramids; but, in many, the hexagonal plane terminating the pyramid is so small, that it renders its total suppression probable.

This decrease necessarily produces a single pyramid, as abovementioned; nevertheless, there are instances of crystals of Corundum, belonging to the variety where the terminal planes make, with the planes of the pyramid, a solid angle of about 100°, in which, two pyramids of the same dimensions, having their summit replaced by a small hexagonal plane, are placed base to base.

I have also observed, among the crystals of the obtuse variety abovementioned, in Mr. GREVILLE's collection, an instance of the decrease taking place by several rows, on one three-sided pyramid of the primitive rhomboid, and by single rows on the other. Consequently, the crystal is a short regular hexaedral prism, terminating on one end only by an hexaedral pyramid; the planes of which, as well as of the prism, are alternately broad and narrow, and almost perfect; its apex being replaced by a very small plane.

I shall conclude, by mentioning a variety of Corundum, described by the Abbé HAUY, in the *Journal des Mines*, No. 28; in which, the edges of the terminal planes of the hexaedral prism are replaced by planes which form an angle of 116° 31′, with the terminal planes; but, in the numerous collection of Mr. GREVILLE, I have not seen this variety. One crystal had an appearance of such planes; but, on examination, it was clearly accidental. The authority of the Abbé HAUY, in crystallography, is so great, that the existence of such modification ought not to be denied, without further examination; though I cannot in this instance adopt it: he derives this variety, which he calls subpyramidal, from a decrease of three rows of molecules, at the angles of the base of the two pyramids of the primitive

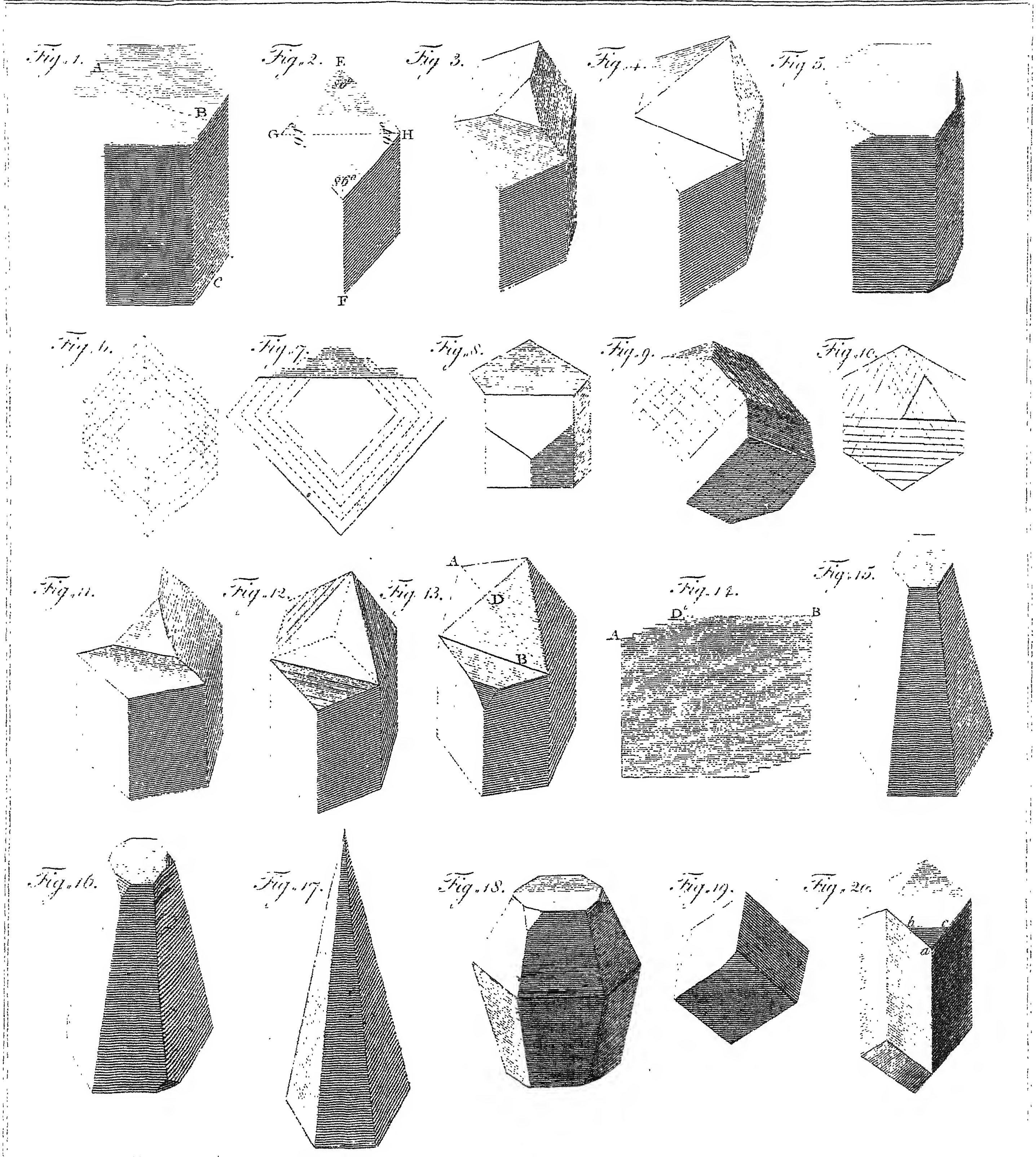


Fig. 1.
A
B
C
Fig. 2.
E
80°
G
H
86°
F
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.
Fig. 8.
Fig. 9.
Fig. 10.
Fig. 11.
Fig. 12.
Fig. 13.
A
D
B
Fig. 14.
D
B
A
Fig. 15.
Fig. 16.
Fig. 17.
Fig. 18.
Fig. 19.
Fig. 20.
b
c
a

rhomboid; and he seems to attribute the same formation to the pyramidal variety with double pyramid, which he supposes may exist.

The primitive crystals, and the first and second modifications of Corundum, are from the Peninsula of India. The third modification, or the pyramidal variety, is from China; nothing approaching this form being among the specimens which Mr. GREVILLE received from the Peninsula of India.

The preceding observations, and particularly the last mentioned modification of Corundum, compared with the best descriptions of the sapphire, suggest the further examination of the degree of connection, if not of identity, of these Oriental stones.

In both, the hexaedral pyramids are usually incomplete in their apex, and they vary in acuteness. I have stated the degree in which the solid angles of the pyramid (taken as complete) vary, in Corundum, to be from 20° to 40°.

ROMÉ DE L'ISLE states, that the sapphire varies from 20° to 30°. The Abbé HAUY (*Journal de Physique*, Aug. 1793,) mentions two varieties of the sapphire, one measuring at the solid angle of the pyramid 40° 6′, the other 57° 24′. I never saw a sapphire with so obtuse an angle as the last; but many, whose angle at the top, if the pyramid had been complete, would have been the same as that of the Corundum. Besides the analogy between the crystals of Corundum and the sapphire, by the union of two hexaedral pyramids at their base, it also exists by the measure of their angles; and both substances are subject to the same irregularity, sometimes appearing as a single hexaedral pyramid, and sometimes as an hexaedral

prism; moreover, the sapphire sometimes has on its solid angles, alternately, the same triangular planes, (fig. 5.) and also the prominent triangles on the planes of the extremities, (fig. 10.) which often appear in the crystals of Corundum. The Abbé HAUY, in the *Journal de Physique*, August, 1793, names this variety, *Orientale Enneagone*, which is represented in the annexed Plate, (fig. 18.) and says, that the small triangular planes make, with the terminal planes, an angle of 122° 18′; and, in the description of the same triangular planes in the Corundum, (fig. 16.) it appears, that these planes are the remains of the planes of the primitive rhomboid, and form, with the terminal planes, an angle of 122° 34′.

Perhaps the rhomboidal crystal, which ROME′ DE L'ISLE had given as one of the forms of the sapphire, should be restored to it. He had examined it at M. JACQUEMIN'S, jeweller to the crown, (*Cristallographie*, 1. edit. p. 221.) and he suppressed it in his second edition, but often expressed to me his regret in having made the alteration. I have before me a letter from that celebrated naturalist, dated September, 1784,* in which he inclosed, for my opinion, a copy of a letter he had received from Mr. WERNER, with models of some crystals; among them, two called by him rubies; one a rhomboid, of which the angles of the summit are substituted by planes, (fig. 19.) the other is precisely the same as fig. 3, 4, and 5, of the annexed Plate.

The following is a translation of ROME′ DE L'ISLE'S words: "The first of these rubies has exactly the same form as I have "represented in Plate IV. fig. 60. of my *Cristallographie*, viz.

* A letter to the same effect was written to M. LA METHERIE, and published in the *Journal de Physique*, May, 1787.

"a rhomboidal parallelopiped, truncated at each of its obtuse "angles by an equilateral triangular plane.

"You will have a correct idea of the other crystal, if you "suppose the crystal represented in Plate IV. fig. 87. truncated "at each of the summits of its pyramids, by an equilateral tri- "angular plane, as in the preceding modification, but deeper, "and in so great a degree, that the three rhombic planes of each "pyramid disappear, with the exception of three isosceles tri- "angles; this modification differs from the first, only by the "hexaedral prism, and the deeper truncature at the summits of "the pyramids."

It is therefore clear, that if the primitive rhomboid of Corundum decreased only at the superior angles of its laminæ, it would exhibit exactly the first of these varieties of Mr. Werner's ruby, as in the annexed fig. 19.

As to the second variety of Mr. Werner's ruby, it is equally clear, if in fig. 87, referred to by Romé de l'Isle, (represented by the annexed fig. 20.) no more of the pyramid was left than the three small triangles *b*, *a*, *c*, there would be precisely one of the forms of Corundum before described, to which the annexed figure 5 belongs.

It may perhaps be objected, that the laminæ appear to be parallel to the terminal planes, in the sapphire, and inclined, in the Corundum. There are crystals of Corundum, in which, very frequently, the laminæ appear parallel to the terminal plane; I was at first, and for some time, deceived by that appearance. In other Corundum crystals, the laminæ appear to be parallel to the prismatic planes; and, to conclude the instances of analogy, the superposition of rhomboidal laminæ is sometimes observable

in Oriental rubies and sapphires. It was by this appearance, Mr. GREVILLE was led to try the effect of cutting the forementioned stones *en cabochon;* whereby a similar effect of triple reflection, which formed stars of six rays from a common centre, was produced in the Oriental ruby, in the sapphire, and in the Corundum.

It is to be lamented that Mr. WERNER did not send, with his models, the specific gravity of each of the rubies, and the measures of their angles: we should then have had data to decide, whether the rubies sent by Mr. WERNER were, as I suppose them to be, Oriental rubies, or sapphires; and, with equal certainty, whether the parallelopipedal rhomboid corresponds precisely with that of the Corundum; by this, the perfect identity, or analogy, between the Corundum and the sapphire, would have been no longer doubtful.

Table of the specific Gravity of the Corundum, Sapphire, Topaz, Ruby, and Diamond, on different Authorities.

Corundum.

Hatchett and Greville	2,768	* Matrix of Corundum. Coast.
H. and G. -	2,785	* Lump of Corundum. Coast.
Klaproth - -	3,075	
Klaproth -	3,710	
Blumenbach -	3,808	
Brisson -	3,873	
Hatchett and Greville	3,876	* Corone. Bengal.
Lichtenberg -	3,908	
Gross - -	3,935	
Hatchett and Greville	3,950	* Crystal of Corundum. Coast.
H. and G. -	3,954	* Ditto with vitreous cross fracture. Coast.
H. and G. -	3,959	* Ruby-coloured. Coast.
H. and G. -	3,959	* Chatoyant. China.
H. and G. -	3,962	* Crystal. China.
Klaproth - -	4,180	

Sapphire.

Brisson - -	3,130	Brasilian; probably a topaz.
Bergman -	3,650 †	
Quist - -	3,800 †	
Bergman -	3,940 †	
Klaproth -	3,950 †	
Bergman -	3,974 †	

Brisson	- -	3,991 †	White Oriental.
Brisson	-	3,991 †	
Blumenbach	-	3,991 †	Blue.
Hatchett and Greville		4,000 †	* Greyish star-stone.
Werner	-	4,000 †	
Blumenbach	-	4,010 †	
Hatchett and Greville		4,035 †	Blue star-stone.
Brisson	- -	4,076 †	From Puy-en-Velay.
Blumenbach	-	4,083 †	Crimson.
Hatchett and Greville		4,083 †	* Pale blue crystal.
Muschenbroeck	-	4,090 †	
Blumenbach	-	4,100 †	Yellow.
La Metherie	-	4,200 †	
Quist	- -	4,200 †	

Topaz.

La Metherie	-	2,690	Siberia.
Bergman	-	3,460	
Werner	-	3,464	Light blue. Brazil.
Quist	- -	3,500	
Werner	- -	3,521	Eibenstocker.
Brisson	-	3,531	Red. Brazil.
Brisson	- -	3,536	Dark yellow. Brazil.
Werner	-	3,540	Dark yellow. Brazil.
Brisson	- -	3,548	Oriental.
Werner	- -	3,556	Schneckensteiner.
Brisson	-	3,564	Schneckensteiner.
Brisson	-	4,010 †	Oriental.
Bergman	-	4,560 †	

Ruby.

Bergman - -	3,180	
Muschenbroeck -	3,180	
Quist - -	3,400	Spinel.
Blumenbach -	3,454	Ceylon.
Quist - -	3,500	Brazil.
Brisson - -	3,531	Brazil.
Klaproth -	3,570	
Hatchett and Greville	3,571	* Octoedral crystal.
H. and G. -	3,625	* Macle of octoedral crystal.
Blumenbach -	3,645	
Blumenbach -	3,760	
Brisson - -	3,760	
Hatchett and Greville	4,166	* Salam ruby. Star-stone. Coast.
Quist - -	4,200 †	
Bergman -	4,240 †	

Diamond.

Hatchett and Greville	3,356	Perfect crystal.
Wallerius -	3,400	
Hatchett and Greville	3,471	Aggregate crystal.
Cronstedt -	3,500	
Muschenbroeck -	3,518	
La Metherie -	3,520	
Brisson - -	3,521	
Werner -	3,600	

The mark * distinguishes the specimens in my collection, to which I have referred in the foregoing Paper.

The mark † distinguishes the stones which, from their specific gravity, I think belong to the genus of Corundum.

The generic name Corundum, I am in the habit of giving to those sorts which have a sparry or a granulated fracture. When Corundum has a vitreous cross fracture, I call it sapphire; and distinguish its varieties by their colours, white, red, blue, yellow, green; and by the accidental reflection of light from their laminæ: when in one direction, I call the sapphire *chatoyant;* when the reflection is compounded of rays which intersect each other, and appear to diverge from a common centre, I call them star-stones, as red, blue, or greyish star-stones, or star-sapphires.

XX. *An Inquiry concerning the chemical Properties that have been attributed to Light.* By Benjamin *Count of* Rumford, *F. R. S. M. R. I. A.*

Read June 14, 1798.

In the Second Part of my Seventh Essay, (on the propagation of heat in fluids,) I have mentioned the reasons which had induced me to doubt of the existence of those chemical properties in light that have been attributed to it, and to conclude, that all those visible changes produced in bodies by exposure to the action of the sun's rays, are effected, not by any chemical combination of the matter of light with such bodies, but merely by the heat which is generated, or excited, by the light that is absorbed by them.

As the decision of this question is a matter of great importance to the advancement of science, and particularly to chemistry, and as the subject is in many respects curious and interesting, it has often employed my thoughts in my leisure hours; and I have spent much time in endeavouring to contrive experiments, from the unequivocal results of which the truth might be made to appear. Though I have not been so successful in these investigations as I could wish, yet I cannot help flattering myself, that an account of the results of some of my late experiments will be thought sufficiently interesting to merit the attention of the Royal Society.

Having found that gold, or silver, might be melted by the heat (invisible to the sight) which exists in the air, at the distance of more than an inch above the point of the flame of a wax-candle, (see my Seventh Essay, Part II. page 350.) I was curious to know what effect this heat would produce on the oxides of those metals.

Experiment No. 1. Having evaporated to dryness a solution of fine gold in aqua regia, I dissolved the residuum, in just as much distilled water as was necessary in order that the solution (which was of a beautiful yellow colour) might not be disposed to crystallize; and, wetting the middle of a piece of white taffeta riband, $1\frac{1}{2}$ inch wide, and about eight inches long, in this solution, I held the riband, with both my hands, stretched horizontally over the clear bright flame of a wax candle; the under side of the riband being kept at the distance of about $1\frac{1}{2}$ inch above the point of the flame. The result of this experiment was very striking. That part of the riband which was directly over the point of the flame, began almost immediately to emit steam in dense clouds; and, in about 10 seconds, a circular spot, about $\frac{3}{4}$ of an inch in diameter, having become nearly dry, a spot of a very fine purple colour, approaching to crimson, suddenly made its appearance in the middle of it, and, spreading rapidly on all sides, became, in one or two seconds more, nearly an inch in diameter.

By moving the riband, so as to bring, in their turns, all the parts of it which had been wetted with the solution to be exposed to the action of the current of hot vapour that arose from the burning candle, all those parts which had been so wetted, were tinged with the same beautiful purple colour.

This colour, which was uncommonly brilliant, passed quite

through the riband, and I found the stain to be perfectly indelible. I endeavoured to wash it out; but nothing I applied to it, and among other things I tried super-oxygenated marine acid, appeared in the smallest degree to diminish its lustre. The hue was not uniform, but varied from a light crimson to a very deep purple, approaching to a reddish brown.

I searched, but in vain, for traces of revived gold, in its reguline form and colour; but, though I could not perceive that the riband was gilded, it had all the appearance of being covered with a thin coating of the most beautiful purple enamel, which, in the sun, had a degree of brilliancy that was sometimes quite dazzling.

Experiment No. 2. A piece of the riband which had been wetted with the aqueous solution of the oxide, was carefully dried in a dark closet, and was then exposed, dry, over the flame of a burning wax candle. The part of the riband which had been wetted with the solution, (and which on drying had acquired a faint yellow colour,) was tinged of the same bright purple colour as was produced in the last-mentioned experiment, when the riband was exposed wet to the action of the heat.*

Experiment No. 3. A piece of the riband which had been wetted with the solution, and dried in the dark, was now wetted with distilled water, and exposed wet to the action of the ascending current of hot vapour which arose from the burning candle: the purple stain was produced as before, which extended as far

* We shall hereafter find reason to conclude, that the success of this experiment, or the appearance of the purple tinge, was owing to the watery vapour which existed in the hot current that ascended from the flame of the candle.

as the riband had been wetted with the solution, but no farther.

I afterwards varied this experiment in several ways, sometimes using paper, sometimes fine linen, and sometimes fine cotton cloths, instead of the silk riband; but nearly the same tinge was produced, whatever the substance was that was made to imbibe the aqueous solution of the metallic oxide.

Similar experiments, and with similar results, were likewise made with pieces of riband, fine linen, cotton, paper, *&c.* wetted in an aqueous solution of nitrate of silver; with this difference, however, that the tinge produced by this metallic oxide, instead of being of a deep purple, inclining to crimson, was of a very dark orange colour, or rather of a yellowish brown.

In order to discover whether the purple tinge, in the experiments with the oxide of gold, was occasioned by the *heat* communicated by the ascending current of hot vapour, or by the *light* of the candle, I made the following experiment, the result of which, I conceive to have been decisive.

Experiment No. 4. A piece of riband was wetted with the aqueous solution of the oxide of gold, and held vertically by the side of the clear flame of a burning wax candle, at the distance of less than half an inch from the flame.

The riband was dried, but its colour was not in the smallest degree changed.

When it was held a few seconds within about $\frac{1}{8}$ of an inch of the flame, a tinge of a most beautiful crimson colour, in the form of a narrow vertical stripe, was produced.

The heat which existed at that distance from the flame, *on the side of it*, where this coloured stripe was produced, was sufficiently intense, as I found by experiment, to melt very fine silver wire, flatted, such as is used in making silver lace.

The objects I had in view in the following experiments, are too evident to require any particular explanation.

Experiment No. 5. Two like pieces of riband were wetted at the same time in the solution, and suspended, while wet, in two thin phials, A and B, of very transparent and colourless glass; the mouths of the phials being left open. Both these phials were placed in a window which fronted the south; that distinguished by the letter A being exposed naked to the direct rays of a bright sun; while B was inclosed in a cylinder of pasteboard, painted black within and without, and closed with a fit cover, and consequently remained in perfect darkness.

In a very few minutes, the riband in the phial A began sensibly to change its colour, and to take a purple hue; and, at the end of five hours, it had acquired a deep crimson tint throughout.

The phial B was exposed in the window, in its dark cylindrical cover, three days; but there was not the smallest appearance of any change of colour in the silk.

Experiment No. 6. Two small parcels of *magnesia alba*, in an impalpable powder, (about half as much in each as could be made to lie on a shilling,) were placed in heaps, in two China plates, A and B, and thoroughly moistened with the before-mentioned aqueous solution of the oxide of gold. Both plates were placed in the same window; the moistened earth in the plate A being exposed naked to the sun's rays; while

that in the plate B was exactly covered with a tea-cup, turned upside down, which excluded all light.

The *magnesia alba* in the plate A, which was exposed to the strong light of the sun, began almost immediately to change colour, taking a faint violet hue, which by degrees became more and more intense, and in a few hours ended in a deep purple; while that in the plate B, which was kept in the dark, retained the yellowish cast it had acquired from the solution, without the smallest appearance of change.

Experiment No. 7. A small parcel of *magnesia alba*, placed on a china plate, having been moistened with the aqueous solution of the oxide of gold, and thoroughly dried in a dark closet, was now exposed, *in this dry state*, to the action of the direct rays of a very bright sun.

It had been exposed to this strong light above half an hour, before its colour began to be *sensibly changed*; and, at the end of three hours, it had acquired only a very faint violet hue.

Being now thoroughly wetted with distilled water, it changed colour very rapidly, and soon came to be of a deep purple tint, approaching to crimson.

Experiment No. 8. A piece of white taffeta riband, which had been wetted with the solution, and thoroughly dried in the dark, was suspended in a clean dry phial of very fine transparent glass; and the phial, being well stopped with a dry cork, was exposed to the strong light of a bright sun.

After the riband had been exposed, in this manner, to the action of the sun's direct rays about half an hour, there were here and there some faint appearances of a change of its colour; but it showed no disposition to take that deep purple hue which

the riband had always acquired, when exposed to the light in the preceding experiments.

On taking the riband out of the phial, and wetting it thoroughly with distilled water, and exposing it again, *while thus wetted*, to the sun's rays, it almost instantly began to change colour, and soon became of a deep purple tint; but, though I examined the surface of the riband with the utmost care, and with a good lens, both during the experiment and after it, I could not perceive the smallest particle of *revived gold*, nor did I see any vestige remaining that appeared to indicate that any had in fact been revived.

This experiment was repeated several times, and always with results which led me to conclude, (what indeed was reasonable to expect,) that light has little effect in changing the colour of metallic oxides, *as long as they are in a state of crystallization.*

The heat which is generated by the absorption of the rays of light must necessarily, *at the moment of its generation* at least, exist in almost infinitely small spaces; and consequently, it is only in bodies that are *inconceivably small* that it can produce durable effects, in any degree indicative of its extreme intensity.

Perhaps the particles of the oxide of gold dissolved in water, are of such dimensions; and it is very remarkable, that the colours produced, in some of my experiments on white ribands, by means of an aqueous solution of the oxide of gold, are precisely the same as are produced from the oxide of that metal, by enamellers, in the intense heat of their furnaces.

As the colouring substance is the same, and as the colours produced are the same, why should we not conclude that the effects are produced in both these cases by the same means, that is to say, by the agency of heat? or, in other words, and

to be more explicit, by exposing the oxide in a certain temperature, at which it becomes disposed to vitrify, or to undergo a change in regard to the quantity of oxygen with which it is combined?

But the results of the following experiments afford still more satisfactory information, respecting the intensity of the heat generated in all cases where light is absorbed, and the striking effects which, under certain circumstances, it is capable of producing.

The facility with which most of the metallic oxides are reduced, in the dry way, by means of charcoal, shows that, at a certain (high) temperature, oxygen is disposed to quit those metals, in order to form a chemical union with the charcoal, or at least with some one of its constituent principles, if it be a compound substance; and hence I concluded, that gold might be revived, *in the moist way*, by means of charcoal, from a solution of its oxide in water, were it possible, under such circumstances, to communicate to the charcoal, and to the oxide, *at the same time*, a degree of heat sufficient for that purpose.

To see if this might not be done by means of light, I made, or rather repeated, the following very interesting experiment.

Experiment No. 9. Into a thin tube of very fine colourless glass, 10 inches long, and $\frac{6}{10}$ of an inch in diameter, closed hermetically at its lower end, I put as many pieces of charcoal, about the size of large peas, as filled the tube to the height of two inches; and, having poured on them as much of the aqueous solution of nitro-muriate of gold as nearly covered them, exposed the tube, with its contents, to the action of the direct rays of a very bright sun.

In less than half an hour, small specks of revived gold, in all

its metallic splendour, began to make their appearance here and there on the surface of the charcoal; and, in six hours, the solution, which at first was of a bright yellow colour, became perfectly *colourless*, AND AS CLEAR AND TRANSPARENT AS THE PUREST WATER.

The surface of the charcoal was, in several places, nearly covered with small particles of revived gold; and the inside of the glass tube, in that part where it was in contact with the upper surface of the contained liquid, was most beautifully gilded.

This gilding of the tube was very splendid, when viewed by reflected light; but, when the tube was placed between the light and the eye, it appeared like a thin cloud, of a greenish blue colour, without the smallest appearance of any metallic splendour.

From the colour, and apparent density of this cloud, I was induced to conclude, that the gilding on the glass was less than *one millionth part of an inch* in thickness.

This interesting experiment was repeated six times, and always with nearly the same result. The gold was completely revived in each of them, and the solution left perfectly colourless; in most of the experiments, however, the sides of the glass were not gilded, all the revived gold remaining attached to the surface of the charcoal.

In two of these experiments, I made use of pieces of charcoal which had been previously boiled several hours in a large quantity of distilled water, and which were introduced *wet*, and *hot*, into the tube, and immediately covered by the solution, to prevent them from imbibing any air; and, in different experiments, the solution was used of different degrees of strength.

I plainly perceived that the experiment succeeded best, that is to say, that the gold was *soonest revived*, in those cases in which the solution was *most diluted*: one of the experiments, however, and which succeeded perfectly, was made with the solution so much condensed, that it was nearly at the point at which it became disposed to crystallize.*

On examining, with a good microscope, the particles of revived gold which remained attached to the surface of the charcoal, after it had been dried, I found them to consist of an infinite number of small scales, separated from each other; not very highly polished, but possessing the true metallic splendour, and a very deep and rich gold colour.

The gold which attached itself to the inside of the glass tube, was in the form of a ring, about $\frac{1}{10}$ of an inch wide, (badly defined however below,) and adhered to the glass with so much obstinacy, as not to be removed by rincing out the tube a great number of times with water; it had, as has already been observed, a very high polish, when seen by reflected light.

Those who enter into the spirit of these investigations, will easily imagine how impatient I must have been, after seeing the results of these experiments, to find out whether gold could be revived from this aqueous solution of its oxide by means of charcoal, *without the assistance of light*, and merely by such a degree of equal heat as could be given to it in the dark. To determine that important question, the following experiment was made.

* This agrees perfectly with the results of similar experiments made by the ingenious and lively Mrs. FULHAME. (See her Essay on Combustion, page 124.)

It was on reading her book, that I was induced to engage in these investigations; and it was by her experiments, that most of the foregoing experiments were suggested.

Experiment No. 10. A cylindrical glass tube, $\frac{6}{10}$ of an inch in diameter, and 10 inches long, closed hermetically at its lower end, and containing a quantity of a diluted aqueous solution of the oxide of gold, mixed with charcoal in broken pieces, about the size of large peas, was put into a fit cylindrical tin case, which was nicely closed with a fit cover; and the glass tube, with its contents, so shut up in the dark, was exposed two hours, in the temperature of 210° of FAHRENHEIT'S scale.

On taking the glass tube out of its tin case, I found the solution *perfectly colourless*, and the revived gold adhering to the surface of the charcoal.

On repeating the experiment, and using the solution nearly saturated with the oxide, the result was precisely the same; the solution being found perfectly colourless, and the revived gold adhering to the surface of the charcoal.

I own fairly, that the results of these experiments were quite contrary to my expectations, and that I am not able to reconcile them with my hypothesis, respecting the causes of the reduction of the oxide, in the foregoing experiments; but, whatever may be the fate of this, or of any other hypothesis of mine, I hope and trust that I never shall be so weak as to feel pain at the discovery of truth, however contrary it may be to my expectations; and still less, to feel a secret wish to suppress experiments, merely because their results militate against my speculative opinions.

It is proper I should observe, that the charcoal used in this last-mentioned experiment had been boiled two hours in distilled water, by which means its pores had been so completely filled with that fluid, that the pieces of it that were

used were specifically heavier than water, and sunk in it, to the bottom of the containing vessel.

Having been so successful in my attempts to reduce the oxide of gold, by means of charcoal, *in the moist way*, I lost no time in making similar experiments with the oxide of silver.

Experiment No. 11. A solution of fine silver, in strong nitrous acid, was evaporated to dryness, and the residuum redissolved in distilled water.

A portion of this solution, (which was perfectly colourless,) diluted with twice as much distilled water, was poured into a phial containing a number of small pieces of charcoal; and the phial, being well closed with a new cork stopple, was exposed to the action of the sun's rays.

In less than an hour, small specks of revived silver began to make their appearance on the surface of the charcoal; and, at the end of two hours, these specks became very numerous, and had increased so much in size, that they were distinctly visible to the naked eye, at the distance of more than three feet. They were very white, and possessed the metallic splendour of silver in so high a degree, that when enlightened by the sun's beams, their lustre was nearly equal to that of very small diamonds.

The phial, which was in the form of a pear, and about $1\frac{1}{2}$ inch in diameter at its bulb, was very thin, and made of very fine colourless glass; the aqueous solution was also perfectly transparent and colourless; and, when the contents of the phial were illuminated by the direct rays of a bright sun, the contrast of the white colour of these little metallic spangles with the black charcoal to which they were fixed, and their

extreme brilliancy, afforded a very beautiful and interesting sight.

As the air had been previously expelled from the charcoal, by boiling it in distilled water, it was specifically heavier than the aqueous solution of the metallic oxide, and consequently remained at the bottom of the bottle.

Experiment No. 12. A phial, as nearly as possible like that used in the last experiment, and containing the same quantity of diluted aqueous solution of nitrate of silver, and also of charcoal, was inclosed in a cylindrical tin box, and exposed one hour to the heat of boiling water, in an apparatus used for boiling potatoes in steam, for the table.

The result of this experiment was uncommonly striking. The surface of the charcoal was covered with a most beautiful metallic vegetation; small filaments of revived silver, resembling fine flatted silver wire, pushing out from its surface, in all directions!

Some of these metallic filaments were above one-tenth of an inch in length. On agitating the contents of the phial, they were easily detached from the surface of the charcoal; to which they seemed to adhere but very slightly.

These experiments were repeated several times, and always with precisely the same results.

When the oxide of gold was reduced in this way, the revived metal appeared under the form of small scales, adhering firmly to the surface of the charcoal. May not the difference of the forms under which gold and silver are revived from their oxides, in this process, be owing to the difference of the specific gravities of those metals?

The following experiments, which were first suggested by an accident, were made with a view to investigate still farther the causes of those effects which have been attributed to the supposed chemical properties of light.

Having accidentally put away two small phials, each containing a quantity of aqueous solution of the oxide of gold and sulphuric ether, in each of which the ether had extracted the gold completely from the solution, as was evident by the yellow colour of the solution having been transferred to the ether, and the solution being left colourless; in one of the phials, which happened to stand in a window in which there was occasionally a strong light, (though the direct rays of the sun never fell on it,) I found, in about three weeks, that the oxide was almost entirely reduced; the revived gold appearing in all its metallic splendour, in the form of a thin pellicle, swimming on the surface of the aqueous liquor in the phial, and the colour of the ether which reposed on it having become quite faint; while no visible change had been produced in the contents of the other phial, which had stood in a dark corner of the room.

As these appearances induced me to suspect, or rather strengthened the suspicions I had before conceived, that the separation of gold from ether, under its metallic form, when a solution of its oxide is mixed with that fluid, is always effected by a reduction of the oxide by means of light, I made the following experiment, with a view to the farther investigation of that matter.

Experiment No. 13. Into a small pear-like phial, of very fine transparent glass, I put equal quantities of an aqueous solution of the muriatic oxide of gold and sulphuric ether; and the phial,

which was about half filled, being closed with a good cork, well secured in its place, was exposed to the action of the direct rays of a bright sun.

A pellicle of revived gold, in all its metallic splendour, began almost immediately to be formed on the surface of the aqueous liquid, and soon covered it entirely; and, at the end of two hours, the whole of the oxide was completely reduced, as was evident from the appearance of the ether, which became *perfectly colourless.*

On shaking the phial, the metallic pellicle which covered the surface of the aqueous liquid was broken into small pieces, which had exactly the appearance of leaf gold, possessing the true colour, and all the metallic brilliancy, of that metal.

On suffering the phial to stand quiet, the aqueous liquor and the ether separated, and most of the broken pieces of the thin sheet of gold descended to the bottom of the phial: the remainder of them floated on the surface of the aqueous liquid; and the ether, as well as the aqueous liquid, appeared to be perfectly transparent and *colourless.*

By the length of time which was required for the ether and the aqueous liquid to separate, I thought I could perceive that the ether had lost something of its fluidity; but, as this was an event I expected, it is the more likely, on that account, that I was deceived, when I imagined I saw proofs of its having taken place.

On removing the cork, after the contents of the bottle had been suffered to cool, there was no appearance of any considerable quantity of air, or other permanently elastic fluid, having been either generated or absorbed, during the experiment.

Finding that the oxide of gold might be so completely and

so expeditiously reduced, by means of ether, I conceived it might be possible to perform that chemical process, *in the moist way*, by means of essential oils; and this conjecture proved to be well founded.

Experiment No. 14. Upon a quantity of a diluted aqueous solution of nitro-muriate of gold, in a small pear-like phial, about $1\frac{1}{2}$ inch in diameter at its bulb, was poured a small quantity of etherial oil of turpentine, just as much as was sufficient to cover the aqueous solution to the height of $\frac{2}{10}$ of an inch; and the phial, being well closed with a good cork, well secured, was exposed one hour to the heat of boiling water in a steam-vessel.

The gold was revived, appearing in the form of a splendid pellicle, of a bright gold colour, which floated on the surface of the aqueous liquid. The oil of turpentine, which, at the beginning of the experiment, was as pale and colourless as pure water, had taken a bright yellow hue; and the aqueous fluid, on which it reposed, had entirely lost its yellow colour.

On shaking the phial, its contents were intimately mixed; but, on suffering it to stand quiet, the oil of turpentine soon separated from the aqueous liquid, retaining its bright yellow hue, and leaving the aqueous liquid colourless.

On shaking the phial, *before it had been exposed to the heat*, and mixing its contents, and then suffering it to stand quiet, the oil of turpentine, on taking its place at the top of the aqueous solution, was not found to have acquired any colour; nor was the bright gold colour of the solution found to be at all impaired. When sulphuric ether was used, instead of the oil of turpentine, the effect was in this respect very different.

To find out whether the oil of turpentine used in this expe-

riment, and which had acquired a deep yellow colour, had lost that property by which it effected the reduction of the metallic oxide, I now poured an additional quantity of the aqueous solution of the oxide into the phial, and, shaking the phial, exposed it, with its contents, to the heat of boiling water.

After it had been exposed to this heat about two hours, I examined it, and found, that though a considerable quantity of gold had been revived, yet the aqueous liquid still retained a faint yellow colour.

The oil of turpentine had acquired a deeper and richer gold colour, approaching to orange.

To the contents of the phial, I now added about half as much distilled water, and, mixing the whole by shaking, I exposed the phial again, during two hours, to the heat of boiling water; when the remainder of the oxide was reduced, and the aqueous liquid left perfectly *colourless.*

On repeating this experiment with oil of turpentine, and varying it, by using a solution of the oxide of *silver,* (an aqueous solution of nitrate of silver,) instead of that of *gold,* the result was nearly the same: the metal was revived, and the oil of turpentine acquired a faint greenish-yellow colour.

I also revived the oxides of gold and of silver with *oil of olives,* by a similar process, with the heat of boiling water. The oil of olives used in these experiments lost its transparency, and became deeply coloured; that used in the reduction of the oxide of silver, taking a very deep dirty brown colour, approaching to black; and that employed in reducing the oxide of gold, being changed to a yellowish-brown, with a purple hue.

In the experiment with the oxide of silver, the inside of the phial, in the region where the oil reposed on the aqueous solu-

tion, was beautifully silvered, the revived metal forming a narrow metallic ring, extending quite round the phial; and, in both experiments, small detached pellicles of revived metal were visible in the oil, and adhered in several places to the inside of the phial, forming bright spots, in which the colour of the metal, and its peculiar splendour, were perfectly conspicuous.

Experiment No. 15. As *carbon* is one of the constituent principles of spirit of wine, as well as of essential oils and sulphuric ether, I thought it possible that I might succeed in the reduction of the oxide of gold, by mixing alcohol, with an aqueous solution of nitro-muriate of gold, and exposing the mixture, in a phial well closed, to the heat of boiling water; but the experiment did not succeed.

By pouring upon this mixture a small quantity of oil of olives, and exposing it again to the heat of boiling water, the gold was revived.

Is it not probable, that the reason why the oxide was not reduced by alcohol, is the mobility of those elements, which ought to act on each other, in order that the effect in question may be produced? I have no doubt but the oxide would be reduced, could the alcohol be made to rest on the surface of the aqueous solution, without mixing with it.

I wished to have been able to have collected and examined the elastic fluids, which probably were formed in most of the preceding experiments; but my time was so much taken up with other matters, that I had not leisure to pursue these investigations farther.

In order to see what effects would be produced by the heat generated at the surface of an opaque body, of a nature different from those hitherto used in the reduction of the metallic oxides,

and one that is little disposed to form a chemical union with oxygen, (*magnesia alba,*) when, being immersed in an aqueous solution of the oxide of gold, the rays of the sun were made to impinge on it, I contrived the following experiment.

Experiment No. 16. I took four small thin phials, A, B, C, and D, of very fine glass, and, putting into each of them about five grains of dry *magnesia alba,* I filled the phial A, nearly full, with a saturated aqueous solution of the oxide of gold.

I filled the phial B, in like manner, with some of the same solution, diluted with an equal quantity of distilled water; and the phials C and D were filled with the solution still farther diluted.

These phials, open or without stoppers, were exposed one whole day to the action of the direct rays of a bright sun, their contents being often well mixed together, during that time, by shaking.

The contents of all these phials changed colour, more or less, but they acquired very different hues. The contents of the phial A became of a very deep rich *gold colour,* approaching to *orange,* the earthy sediment being throughout of the same tint.

The contents of the phial B, which were at first of a light straw colour, first changed to a light green, and then to a greenish blue. The phial having been suffered to stand quiet several days, in an uninhabited room, in a retired part of the house, the solution became nearly colourless, and the sediment was found to be of a dirty olive colour.

The colour of the contents of the phials C and D was changed nearly in the same manner; and, having been suffered to stand quiet two or three days, to settle, the solution was found to be quite colourless, and the sediment to be deeply coloured. There

was, however, a very remarkable difference in the hues of the two phials; that of the phial C being of a light greenish-blue; while that in the phial D was indigo, and of so deep a tint, that it might easily have been taken for black.

These appearances were certainly very striking, and well calculated to excite my curiosity; but I am so much engaged in public business, that it is not at present in my power to pursue these inquiries farther. I wish that what I have done may induce others, who have more time to spare, to devote some portion of their leisure to these interesting investigations.

XXI. *Experiments to determine the Density of the Earth. By* Henry Cavendish, *Esq. F.R.S. and A.S.*

Read June 21, 1798.

MANY years ago, the late Rev. JOHN MICHELL, of this Society, contrived a method of determining the density of the earth, by rendering sensible the attraction of small quantities of matter; but, as he was engaged in other pursuits, he did not complete the apparatus till a short time before his death, and did not live to make any experiments with it. After his death, the apparatus came to the Rev. FRANCIS JOHN HYDE WOLLASTON, Jacksonian Professor at Cambridge, who, not having conveniences for making experiments with it, in the manner he could wish, was so good as to give it to me.

The apparatus is very simple; it consists of a wooden arm, 6 feet long, made so as to unite great strength with little weight. This arm is suspended in an horizontal position, by a slender wire 40 inches long, and to each extremity is hung a leaden ball, about 2 inches in diameter; and the whole is inclosed in a narrow wooden case, to defend it from the wind.

As no more force is required to make this arm turn round on its centre, than what is necessary to twist the suspending wire, it is plain, that if the wire is sufficiently slender, the most minute force, such as the attraction of a leaden weight a few inches in diameter, will be sufficient to draw the arm sensibly aside. The weights which Mr. MICHELL intended to use were

8 inches diameter. One of these was to be placed on one side the case, opposite to one of the balls, and as near it as could conveniently be done, and the other on the other side, opposite to the other ball, so that the attraction of both these weights would conspire in drawing the arm aside; and, when its position, as affected by these weights, was ascertained, the weights were to be removed to the other side of the case, so as to draw the arm the contrary way, and the position of the arm was to be again determined; and, consequently, half the difference of these positions would shew how much the arm was drawn aside by the attraction of the weights.

In order to determine from hence the density of the earth, it is necessary to ascertain what force is required to draw the arm aside through a given space. This Mr. MICHELL intended to do, by putting the arm in motion, and observing the time of its vibrations, from which it may easily be computed.*

Mr. MICHELL had prepared two wooden stands, on which the leaden weights were to be supported, and pushed forwards, till they came almost in contact with the case; but he seems to have intended to move them by hand.

As the force with which the balls are attracted by these weights is excessively minute, not more than $\frac{1}{50,000,000}$ of their weight, it is plain, that a very minute disturbing force will be sufficient to destroy the success of the experiment; and, from the following experiments it will appear, that the disturbing

* Mr. COULOMB has, in a variety of cases, used a contrivance of this kind for trying small attractions; but Mr. MICHELL informed me of his intention of making this experiment, and of the method he intended to use, before the publication of any of Mr. COULOMB's experiments.

force most difficult to guard against, is that arising from the variations of heat and cold; for, if one side of the case is warmer than the other, the air in contact with it will be rarefied, and, in consequence, will ascend, while that on the other side will descend, and produce a current which will draw the arm sensibly aside.*

As I was convinced of the necessity of guarding against this source of error, I resolved to place the apparatus in a room which should remain constantly shut, and to observe the motion of the arm from without, by means of a telescope; and to suspend the leaden weights in such manner, that I could move them without entering into the room. This difference in the manner of observing, rendered it necessary to make some alteration in Mr. MICHELL's apparatus; and, as there were some parts of it which I thought not so convenient as could be wished, I chose to make the greatest part of it afresh.

Fig. 1. (Tab. XXIII.) is a longitudinal vertical section through the instrument, and the building in which it is placed. ABCDDCBAEFFE, is the case; x and x are the two balls, which are suspended by the wires hx from the arm $ghmh$, which is itself suspended by the slender wire gl. This arm consists of a slender deal rod hmh, strengthened by a silver

* M. CASSINI, in observing the variation compass placed by him in the Observatory, (which was constructed so as to make very minute changes of position visible, and in which the needle was suspended by a silk thread,) found that standing near the box, in order to observe, drew the needle sensibly aside; which I have no doubt was caused by this current of air. It must be observed, that his compass-box was of metal, which transmits heat faster than wood, and also was many inches deep; both which causes served to increase the current of air. To diminish the effect of this current, it is by all means advisable to make the box, in which the needle plays, not much deeper than is necessary to prevent the needle from striking against the top and bottom.

wire *bgb*; by which means it is made strong enough to support the balls, though very light.*

The case is supported, and set horizontal, by four screws, resting on posts fixed firmly into the ground: two of them are represented in the figure, by S and S; the two others are not represented, to avoid confusion. GG and GG are the end walls of the building. W and W are the leaden weights; which are suspended by the copper rods R*r*P*r*R, and the wooden bar *rr*, from the centre pin P*p*. This pin passes through a hole in the beam HH, perpendicularly over the centre of the instrument, and turns round in it, being prevented from falling by the plate *p*. MM is a pulley, fastened to this pin; and M*m*, a cord wound round the pulley, and passing through the end wall; by which the observer may turn it round, and thereby move the weights from one situation to the other.

Fig. 2. (Tab. XXIV.) is a plan of the instrument. AAAA is the case. SSSS, the four screws for supporting it. *hh*, the arm and balls. W and W, the weights. MM, the pulley for moving them. When the weights are in this position, both conspire in drawing the arm in the direction *h*W; but, when they are removed to the situation *w* and *w*, represented by the dotted lines, both conspire in drawing the arm in the contrary direction *hw*. These weights are prevented from striking the instrument, by pieces of wood, which stop them as soon as they come within $\frac{1}{5}$ of an inch of the

* Mr. MICHELL's rod was entirely of wood, and was much stronger and stiffer than this, though not much heavier; but, as it had warped when it came to me, I chose to make another, and preferred this form, partly as being easier to construct and meeting with less resistance from the air, and partly because, from its being of a less complicated form, I could more easily compute how much it was attracted by the weights.

case. The pieces of wood are fastened to the wall of the building; and I find, that the weights may strike against them with considerable force, without sensibly shaking the instrument.

In order to determine the situation of the arm, slips of ivory are placed within the case, as near to each end of the arm as can be done without danger of touching it, and are divided to 20ths of an inch. Another small slip of ivory is placed at each end of the arm, serving as a vernier, and subdividing these divisions into 5 parts; so that the position of the arm may be observed with ease to 100ths of an inch, and may be estimated to less. These divisions are viewed, by means of the short telescopes T and T, (fig. 1.) through slits cut in the end of the case, and stopped with glass; they are enlightened by the lamps L and L, with convex glasses, placed so as to throw the light on the divisions; no other light being admitted into the room.

The divisions on the slips of ivory run in the direction W*w*, (fig. 2.) so that, when the weights are placed in the positions *w* and *w*, represented by the dotted circles, the arm is drawn aside, in such direction as to make the index point to a higher number on the slips of ivory; for which reason, I call this the positive position of the weights.

FK, (fig. 1.) is a wooden rod, which, by means of an endless screw, turns round the support to which the wire *gl* is fastened, and thereby enables the observer to turn round the wire, till the arm settles in the middle of the case, without danger of touching either side. The wire *gl* is fastened to its support at top, and to the centre of the arm at bottom, by brass clips, in which it is pinched by screws.

In these two figures, the different parts are drawn nearly in

the proper proportion to each other, and on a scale of one to thirteen.

Before I proceed to the account of the experiments, it will be proper to say something of the manner of observing. Suppose the arm to be at rest, and its position to be observed, let the weights be then moved, the arm will not only be drawn aside thereby, but it will be made to vibrate, and its vibrations will continue a great while; so that, in order to determine how much the arm is drawn aside, it is necessary to observe the extreme points of the vibrations, and from thence to determine the point which it would rest at if its motion was destroyed, or the point of rest, as I shall call it. To do this, I observe three successive extreme points of a vibration, and take the mean between the first and third of these points, as the extreme point of vibration in one direction, and then assume the mean between this and the second extreme, as the point of rest; for, as the vibrations are continually diminishing, it is evident, that the mean between two extreme points will not give the true point of rest.

It may be thought more exact, to observe many extreme points of vibration, so as to find the point of rest by different sets of three extremes, and to take the mean result; but it must be observed, that notwithstanding the pains taken to prevent any disturbing force, the arm will seldom remain perfectly at rest for an hour together; for which reason, it is best to determine the point of rest, from observations made as soon after the motion of the weights as possible.

The next thing to be determined is the time of vibration, which I find in this manner: I observe the two extreme points of a vibration, and also the times at which the arm arrives at

two given divisions between these extremes, taking care, as well as I can guess, that these divisions shall be on different sides of the middle point, and not very far from it. I then compute the middle point of the vibration, and, by proportion, find the time at which the arm comes to this middle point. I then, after a number of vibrations, repeat this operation, and divide the interval of time, between the coming of the arm to these two middle points, by the number of vibrations, which gives the time of one vibration. The following example will explain what is here said more clearly.

Extreme Points.	Division.	Time.	Point of rest.	Time of middle of vibration.
27,2		h. ′ ″		h. ′ ″
	25	10 23 4	-	10 23 23
	24	57		
22,1	-	- -	24,6	
27	-	- -	24,7	
22,6	-	- -	24,75	
26,8	-	- -	24,8	
23	-	- -	24,85	
26,6	-	- -	24,9	
	25	11 5 22	-	11 5 22
	24	6 48		
23,4				

The first column contains the extreme points of the vibrations. The second, the intermediate divisions. The third, the time at which the arm came to these divisions; and the fourth, the point of rest, which is thus found: the mean between the first and third extreme points is 27,1, and the mean between this and the second extreme point is 24,6, which is the point of rest, as found by the three first extremes. In like manner, the

point of rest found by the second, third, and 4th extremes, is 24,7, and so on. The fifth column is the time at which the arm came to the middle point of the vibration, which is thus found: the mean between 27,2 and 22,1 is 24,65, and is the middle point of the first vibration; and, as the arm came to 25 at $10^h\ 23'\ 4''$, and to 24 at $10^h\ 23'\ 57''$, we find, by proportion, that it came to 24,65 at $10^h\ 23'\ 23''$. In like manner, the arm came to the middle of the seventh vibration at $11^h\ 5'\ 22''$; and, therefore, six vibrations were performed in $41'\ 59''$, or one vibration in $7'\ 0''$.

To judge of the propriety of this method, we must consider in what manner the vibration is affected by the resistance of the air, and by the motion of the point of rest.

Let the arm, during the first vibration, move from D to B, (Tab. XXIV. fig. 3.) and, during the second, from B to d; Bd being less than DB, on account of the resistance. Bisect DB in M, and Bd in m, and bisect Mm in n, and let x be any point in the vibration; then, if the resistance is proportional to the square of the velocity, the whole time of a vibration is very little altered; but, if T is taken to the time of one vibration, as the diameter of a circle to its semicircumference, the time of moving from B to n exceeds $\frac{1}{2}$ a vibration, by $\frac{\text{T} \times \text{D}d}{8\,\text{B}n}$ nearly; and the time of moving from B to m falls short of $\frac{1}{2}$ a vibration, by as much; and the time of moving from B to x, in the second vibration, exceeds that of moving from x to B, in the first, by $\frac{\text{T} \times \text{D}d \times \text{B}x^2}{4\,\text{B}n^2 \times \sqrt{\text{B}x \times x\delta}}$, supposing D$d$ to be bisected in δ; so that, if a mean is taken, between the time of the first arrival of the arm at x and its returning back to the same point, this mean will be earlier than the true time of its coming to B, by $\frac{\text{T} \times \text{D}d \times \text{B}x^2}{8\,\text{B}n^2 \sqrt{\text{B}x \times x\delta}}$.

The effect of motion in the point of rest is, that when the arm is moving in the same direction as the point of rest, the time of moving from one extreme point of vibration to the other is increased, and it is diminished when they are moving in contrary directions; but, if the point of rest moves uniformly, the time of moving from one extreme to the middle point of the vibration, will be equal to that of moving from the middle point to the other extreme, and moreover, the time of two successive vibrations will be very little altered; and, therefore, the time of moving from the middle point of one vibration to the middle point of the next, will also be very little altered.

It appears, therefore, that on account of the resistance of the air, the time at which the arm comes to the middle point of the vibration, is not exactly the mean between the times of its coming to the extreme points, which causes some inaccuracy in my method of finding the time of a vibration. It must be observed, however, that as the time of coming to the middle point is before the middle of the vibration, both in the first and last vibration, and in general is nearly equally so, the error produced from this cause must be inconsiderable; and, on the whole, I see no method of finding the time of a vibration which is liable to less objection.

The time of a vibration may be determined, either by previous trials, or it may be done at each experiment, by ascertaining the time of the vibrations which the arm is actually put into by the motion of the weights; but there is one advantage in the latter method, namely, that if there should be any accidental attraction, such as electricity, in the glass plates through which the motion of the arm is seen, which should increase the force necessary to draw the arm aside, it would also dimi-

nish the time of vibration; and, consequently, the error in the result would be much less, when the force required to draw the arm aside was deduced from experiments made at the time, than when it was taken from previous experiments.

Account of the Experiments.

In my first experiments, the wire by which the arm was suspended was $39\frac{1}{4}$ inches long, and was of copper silvered, one foot of which weighed $2\frac{4}{10}$ grains: its stiffness was such, as to make the arm perform a vibration in about 15 minutes. I immediately found, indeed, that it was not stiff enough, as the attraction of the weights drew the balls so much aside, as to make them touch the sides of the case; I, however, chose to make some experiments with it, before I changed it.

In this trial, the rods by which the leaden weights were suspended were of iron; for, as I had taken care that there should be nothing magnetical in the arm, it seemed of no signification whether the rods were magnetical or not; but, for greater security, I took off the leaden weights, and tried what effect the rods would have by themselves. Now I find, by computation, that the attraction of gravity of these rods on the balls, is to that of the weights, nearly as 17 to 2500; so that, as the attraction of the weights appeared, by the foregoing trial, to be sufficient to draw the arm aside by about 15 divisions, the attraction of the rods alone should draw it aside about $\frac{1}{10}$ of a division; and, therefore, the motion of the rods from one near position to the other, should move it about $\frac{1}{5}$ of a division.

The result of the experiment was, that for the first 15 minutes after the rods were removed from one near position to the other, very little motion was produced in the arm, and

hardly more than ought to be produced by the action of gravity; but the motion then increased, so that, in about a quarter or half an hour more, it was found to have moved $\frac{1}{2}$ or $1\frac{1}{2}$ division, in the same direction that it ought to have done by the action of gravity. On returning the irons back to their former position, the arm moved backward, in the same manner that it before moved forward.

It must be observed, that the motion of the arm, in these experiments, was hardly more than would sometimes take place without any apparent cause; but yet, as in three experiments which were made with these rods, the motion was constantly of the same kind, though differing in quantity from $\frac{1}{2}$ to $1\frac{1}{2}$ division, there seems great reason to think that it was produced by the rods.

As this effect seemed to me to be owing to magnetism, though it was not such as I should have expected from that cause, I changed the iron rods for copper, and tried them as before; the result was, that there still seemed to be some effect of the same kind, but more irregular, so that I attributed it to some accidental cause, and therefore hung on the leaden weights, and proceeded with the experiments.

It must be observed, that the effect which seemed to be produced by moving the iron rods from one near position to the other, was, at a medium, not more than one division; whereas the effect produced by moving the weight from the midway to the near position, was about 15 divisions; so that, if I had continued to use the iron rods, the error in the result caused thereby, could hardly have exceeded $\frac{1}{30}$ of the whole.

EXPERIMENT 1. Aug. 5.

Weights in midway position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
	11,4	9 42 0			
	11,5	55 0			
	11,5	10 5 0	11,5		

At $10^h\ 5'$, weights moved to positive position.

23,4			
27,6	-	- -	25,82
24,7	-	- -	26,07
27,3	-	- -	26,1
25,1	-	- -	

At $11^h\ 6'$, weights returned back to midway position.

5,					
	11	0 0 48	-	0 1 13	
	12	1 30			
18,2	-	- -	12	- -	14 56
	12	16 29	-	16 9	
	11	17 20			
6,6	-	- -	11,92	- -	14 36
	11	30 24	-	30 45	
	12	31 11			
16,3	-	- -	11,72	- -	15 13
	12	45 58	-	45 58	
	11	47 4			
7,7					

Motion on moving from midway to pos. = 14,32
pos. to midway - - = 14,1
Time of one vibration - - = 14′ 55″

It must be observed, that in this experiment, the attraction of the weights drew the arm from 11,5 to 25,8, so that, if no contrivance had been used to prevent it, the momentum acquired thereby would have carried it to near 40, and would, therefore, have made the balls to strike against the case. To prevent this, after the arm had moved near 15 divisions, I returned the weights to the midway position, and let them remain there, till the arm came nearly to the extent of its vibration, and then again moved them to the positive position, whereby the vibrations were so much diminished, that the balls did not touch the sides; and it was this which prevented my observing the first extremity of the vibration. A like method was used, when the weights were returned to the midway position, and in the two following experiments.

The vibrations, in moving the weights from the midway to the positive position, were so small, that it was thought not worth while to observe the time of the vibration. When the weights were returned to the midway position, I determined the time of the arm's coming to the middle point of each vibration, in order to see how nearly the times of the different vibrations agreed together. In great part of the following experiments, I contented myself with observing the time of its coming to the middle point of only the first and last vibration.

EXPERIMENT II. Aug. 6.

Weights in midway position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference
		h. ′ ″		h. ′ ″	′ ″
	11	10 4 0			
	11	11 0			
	11	17 0			
	11	25 0	11,		

Weights moved to positive position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference
29,3					
24,1	-	- -	26,87		
30	-	- -	27,57		
26,2	-	- -	28,02		
29,7	-	- -	28,12		
26,9	-	- -	28,05		
28,7	-	- -	27,85		
27,1	-	- -	27,82		
28,4					

Weights returned to midway position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference
6					
	12	1 3 50	–	1 4 1	
	13	4 34			
18,5	-	- -	12,37	- -	14 52
	13	18 29	–	18 53	
	12	19 18			
6,5	-	- -	11,67	- -	14 46
	11	33 48	–	33 39	
	12	34 51			
15,2	-	- -	11	- -	13 46
	13	45 8	–	47 25	
	12	46 22			

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
7,1	-	- -	10 75	- -	15 25
	11	2 3 48	-	2 2 50	
	12	5 18			
13,6					

Motion of arm on moving weights from

midway to pos. - = 15,87

pos. to midway - = 15,45

Time of one vibration - - - = 14′ 42″

Experiment III. Aug. 7.

The weights being in the positive position, and the arm a little in motion.

31,5					
29	-	- -	30,12		
31	-	- -	30,02		
29,1					

Weights moved to midway position.

9					
	14	10 34 18	-	10 34 55	
	15	35 8			
20,5	–	- -	14,8	- -	14 44
	15	49 31	-	49 39	
	14	50 27			
9,2	–	- -	14,07	- -	14 38
	14	11 5 7	-	11 4 17	
	15	6 18			

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
17,4	-	- -	13,52	- -	14 47
	14	11 18 46	-	11 19 4	
	13	19 58			
10,1	-	- -	13,3	- -	14 27
	13	33 46	-	33 31	
	14	35 26			
15,6					

Weights moved to positive position.

32				
	28	0 2 48	-	0 2 59
	27	3 56		
23,7	-	- -	27,8	
31,8	-	- -	28,27	
25,8	-	- -	28,62	
	27	44 58	-	47 40
	28	46 50		
31,1				

Motion of the arm on moving weights from
pos. to mid. - . = 15,22
mid. to pos. - = 14,5
Time of one vibration, when in mid. position = 14′ 39″
pos. position = 14 54

These experiments are sufficient to shew, that the attraction of the weights on the balls is very sensible, and are also sufficiently regular to determine the quantity of this attraction pretty nearly, as the extreme results do not differ from each other by more than $\frac{1}{10}$ part. But there is a circumstance in them, the reason of which does not readily appear, namely, that the effect of the attraction seems to increase, for half an

hour, or an hour, after the motion of the weights; as it may be observed, that in all three experiments, the mean position kept increasing for that time, after moving the weights to the positive position; and kept decreasing, after moving them from the positive to the midway position.

The first cause which occurred to me was, that possibly there might be a want of elasticity, either in the suspending wire, or something it was fastened to, which might make it yield more to a given pressure, after a long continuance of that pressure, than it did at first.

To put this to the trial, I moved the index so much, that the arm, if not prevented by the sides of the case, would have stood at about 50 divisions, so that, as it could not move farther than to 35 divisions, it was kept in a position 15 divisions distant from that which it would naturally have assumed from the stiffness of the wire; or, in other words, the wire was twisted 15 divisions. After having remained two or three hours in this position, the index was moved back, so as to leave the arm at liberty to assume its natural position.

It must be observed, that if a wire is twisted only a little more than its elasticity admits of, then, instead of setting, as it is called, or acquiring a permanent twist all at once, it sets gradually, and, when it is left at liberty, it gradually loses part of that set which it acquired; so that if, in this experiment, the wire, by having been kept twisted for two or three hours, had gradually yielded to this pressure, or had begun to set, it would gradually restore itself, when left at liberty, and the point of rest would gradually move backwards; but, though the experiment was twice repeated, I could not perceive any such effect.

The arm was next suspended by a stiffer wire.

EXPERIMENT IV. Aug. 12.

Weights in midway position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ' "		h. ' "	' "
	21,6	9 30 0			
	21,5	52 0			
	21,5	10 13 0	21,5		
Weights moved from midway to positive position.					
27,2					
22,1	-	- -	24,6		
27	-	- -	24,67		
22,6	-	- -	24,75		
26,8	-	- -	24,8		
23,0	-	- -	24,85		
26,6	-	- -	24,9		
23,4	-				
Weights moved to negative position.					
15					
	17	19 25	-	10 20 31	
	19	20 41			
22,4	-	- -	18,72	- -	7 0
	20	26 45	-	27 31	
	19	27 22			
15,1	-	- -	18,52	- -	6 57
	19	35 1	-	34 28	
	20	48			
21,5	-	- -	18,35	- -	7 23
	20	40 23	-	41 51	
	19	41 18			
15,3	-	- -	18,22	- -	6 48
	18	48 36	-	48 39	
	19	49 24			
20,8	-	- -	18,1	- -	6 58
	19	54 45	-	55 37	
	18	55 45			
15,5					

Weights moved to positive position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
31,3					
	25	11 10 25	-	11 10 40	
	23	11 3			
17,1	-	- -	24,02	- -	7 3
	22	17 6	-	17 43	
	23	26			
30,6	-	- -	24,17	- -	7 1
	25	24 33	-	24 44	
	23	25 17			
18,4	-	- -	24,32	- -	7 5
	23	31 21	-	31 49	
	25	32 9			
29,9	-	- -	24,4	- -	6 59
	25	38 39	-	38 48	
	23	39 31			
19,4	-	- -	24,5	- -	7 6
	23	45 16	-	45 54	
	25	46 12			
29,3					

Motion of arm on moving weights from
midway to pos. - = 3,1
pos. to neg. - = 6,18
neg. to pos. - = 5,92
Time of one vibration in neg. position - = 7′ 1″
pos. position - = 7 3

EXPERIMENT V. Aug. 20.

The weights being in the positive position, the arm was made to vibrate, by moving the index.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
29,6					
21,1	-	- -	25,2		
29,	-	- -	25,17		
21,6					
Weights moved to negative position.					
22,6					
	20	10 22 47	-	10 23 11	
	19	23 30			
16,3	-	- -	19,27		
21,9	-	- -	19,15		
16,5	-	- -	19,1		
21,5	-	- -	19,07		
16,8	-	- -	19,07		
21,2	-	- -	19,07		
17,1	-	- -	19,05		
20,8	-	- -	19,02		
17,4	-	- -	19,05		
20,6	-	- -	19,02		
	20	11 32 16	-	11 33 53	
	19	33 58			
17,5	-	- -	18,97		
					7 13
	19	41 16	-	41 6	
	20	43 0			
20,3					

Weights moved to positive position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
20,2					
	24	11 49 10	-	11 49 37	
	26	50 19			
29,4	-	- -	24,95	- -	7 7
	26	56 15	-	56 44	
	25	47			
20,8	-	- -	24,92		
28,7	-	- -	24,87		
21,3	-	- -	24,85		
28,1	-	- -	24,75		
21,5	-	- -	24,67		
27,6	-	- -	24,67		
22	-	- -	24,7		
	24	0 45 48	-	0 46 21	
	25	46 43			
27,2	-	- -	24,7	- -	7 1
	25	53 11	-	53 22	
	24	54 9			
22,4					

Motion of arm on moving weights from

pos. to neg. - - $= 5{,}9$

neg. to pos. - - $= 5{,}98$

Time of one vibration, when weights are in

neg. position - - $= 7' \, 5''$

pos. position - - $= 7 \;\; 5$

In the fourth experiment, the effect of the weights seemed to increase on standing, in all three motions of the weights, conformably to what was observed with the former wire; but, in

the last experiment, the case was different; for though, on moving the weights from positive to negative, the effect seemed to increase on standing, yet, on moving them from negative to positive, it diminished.

My next trials were, to see whether this effect was owing to magnetism. Now, as it happened, the case in which the arm was inclosed, was placed nearly parallel to the magnetic east and west, and therefore, if there was any thing magnetic in the balls and weights, the balls would acquire polarity from the earth; and the weights also, after having remained some time, either in the positive or negative position, would acquire polarity in the same direction, and would attract the balls; but, when the weights were moved to the contrary position, that pole which before pointed to the north, would point to the south, and would repel the ball it was approached to; but yet, as repelling one ball towards the south has the same effect on the arm as attracting the other towards the north, this would have no effect on the position of the arm. After some time, however, the poles of the weight would be reversed, and would begin to attract the balls, and would therefore produce the same kind of effect as was actually observed.

To try whether this was the case, I detached the weights from the upper part of the copper rods by which they were suspended, but still retained the lower joint, namely, that which passed through them; I then fixed them in their positive position, in such manner, that they could turn round on this joint, as a vertical axis. I also made an apparatus, by which I could turn them half way round, on these vertical axes, without opening the door of the room.

Having suffered the apparatus to remain in this manner for

a day, I next morning observed the arm, and, having found it to be stationary, turned the weights half way round on their axes, but could not perceive any motion in the arm. Having suffered the weights to remain in this position for about an hour, I turned them back into their former position, but without its having any effect on the arm. This experiment was repeated on two other days, with the same result.

We may be sure, therefore, that the effect in question could not be produced by magnetism in the weights; for, if it was, turning them half round on their axes, would immediately have changed their magnetic attraction into repulsion, and have produced a motion in the arm.

As a further proof of this, I took off the leaden weights, and in their room placed two 10-inch magnets; the apparatus for turning them round being left as it was, and the magnets being placed horizontal, and pointing to the balls, and with their north poles turned to the north; but I could not find that any alteration was produced in the place of the arm, by turning them half round; which not only confirms the deduction drawn from the former experiment, but also seems to shew, that in the experiments with the iron rods, the effect produced could not be owing to magnetism.

The next thing which suggested itself to me was, that possibly the effect might be owing to a difference of temperature between the weights and the case; for it is evident, that if the weights were much warmer than the case, they would warm that side which was next to them, and produce a current of air, which would make the balls approach nearer to the weights. Though I thought it not likely that there should be sufficient difference, between the heat of the weights and case, to have

any sensible effect, and though it seemed improbable that, in all the foregoing experiments, the weights should happen to be warmer than the case, I resolved to examine into it, and for this purpose removed the apparatus used in the last experiments, and supported the weights by the copper rods, as before; and, having placed them in the midway position, I put a lamp under each, and placed a thermometer with its ball close to the outside of the case, near that part which one of the weights approached to in its positive position, and in such manner that I could distinguish the divisions by the telescope. Having done this, I shut the door, and some time after moved the weights to the positive position. At first, the arm was drawn aside only in its usual manner; but, in half an hour, the effect was so much increased, that the arm was drawn 14 divisions aside, instead of about three, as it would otherwise have been, and the thermometer was raised near $1^\circ\frac{1}{2}$; namely, from 61° to $62^\circ\frac{1}{2}$. On opening the door, the weights were found to be no more heated, than just to prevent their feeling cool to my fingers.

As the effect of a difference of temperature appeared to be so great, I bored a small hole in one of the weights, about three-quarters of an inch deep, and inserted the ball of a small thermometer, and then covered up the opening with cement. Another small thermometer was placed with its ball close to the case, and as near to that part to which the weight was approached as could be done with safety; the thermometers being so placed, that when the weights were in the negative position, both could be seen through one of the telescopes, by means of light reflected from a concave mirror.

Experiment VI. Sept. 6.

Weights in midway position.

Extreme points.	Divisions.	Time.	Point of rest.	Thermometer in Air.	Thermometer in Weight.
		h ′			
	18,9	9 43	-	55,5	
	18,85	10 3	18,85		
Weights moved to negative position.					
13,1	-	10 12	-	55,5	55,8
18,4	-	18	15,82		
13,4	-	25			
missed.					
13,6	-	39	-	55,5	55,8
17,6	-	46	15,65		
13,8	-	53	15,65		
17,4	-	11 0	15,65		
14,0	-	7	15,65		
17,2	-	14	-	55,5	
Weights moved to positive position.					
25,8	-	23			
17,5	-	30	21,55		
25,4	-	37	21,6		
18,1	-	44	21,65		
25,0	-	51			
missed.					
24,7	-	0 5			
19,	-	12	21,77		
24,4	-	19			

Motion of arm on moving weights from midway to $- = 3{,}03$

$- \text{ to } + = 5{,}9$

EXPERIMENT VII. Sept. 18.

Weights in midway position.

Extreme points.	Divisions.	Time.	Point of rest.	Thermometer in Air.	Thermometer in Weight.
		h ′			
	19,4	8 30	-	56,7	
	19,4	9 32	-	56,6	
Weights moved to negative position.					
13,6	-	40	-	-	57,2
18,8	-	47	16,25		
13,8	-	54			
Eight extreme points missed.					
16,9	-	10 58			
14,5	-	11 5	15,62		
16,6	-	12			
Weights moved to positive position.					
26,4	-	20	-	56,5	
17,2	-	28	21,72		
26,1	-	35			
Four extreme points missed.					
19,3	-	0 10			
25,1	-	17	22,3		
19,7	-	24			

Motion of arm on moving weights from midway to $- = 3,15$

$- \text{ to } + = 6,1$

EXPERIMENT VIII. Sept. 23.

Weights in midway position.

Extreme points.	Divisions.	Time.	Point of rest.	Thermometer	
				in Air.	in Weight.
		h ′			
	19,3	9 46	-	53,1	
	19,2	10 45	19,2	53,1	
Weights moved to negative position.					
13,5	-	56	-	-	53,6
18,6	-	11 3	16,07		
13,6	-	10			
Four extreme points missed.					
17,4	-	44			
14,1	-	51	15,7		
17,2	-	58	-	-	53,6
Weights moved to positive position.					
15,7	-	0 1			
26,7	-	8	21,42		
16,6	-	15	-	53,15	
Two extreme points missed.					
25,9	-	36			
18,1	-	43	21,9		
25,5	-	50			

Motion of arm on moving weights from midway to $- = 3,13$

$- \text{ to } + = 5,72$

In these three experiments, the effect of the weight appeared to increase from two to five tenths of a division, on standing an hour; and the thermometers shewed, that the weights were three or five tenths of a degree warmer than the air close to the case. In the two last experiments, I put a lamp into the room, over night, in hopes of making the air warmer than the weights, but without effect, as the heat of the weights exceeded that of the air more in these two experiments than in the former.

On the evening of October 17, the weights being placed in the midway position, lamps were put under them, in order to warm them; the door was then shut, and the lamps suffered to burn out. The next morning it was found, on moving the weights to the negative position, that they were $7°\frac{1}{2}$ warmer than the air near the case. After they had continued an hour in that position, they were found to have cooled $1°\frac{1}{2}$, so as to be only 6° warmer than the air. They were then moved to the positive position; and in both positions the arm was drawn aside about four divisions more, after the weights had remained an hour in that position, than it was at first.

May 22, 1798. The experiment was repeated in the same manner, except that the lamps were made so as to burn only a short time, and only two hours were suffered to elapse before the weights were moved. The weights were now found to be scarcely 2° warmer than the case; and the arm was drawn aside about two divisions more, after the weights had remained an hour in the position they were moved to, than it was at first.

On May 23, the experiment was tried in the same manner, except that the weights were cooled by laying ice on them; the ice being confined in its place by tin plates, which, on

moving the weights, fell to the ground, so as not to be in the way. On moving the weights to the negative position, they were found to be about 8° colder than the air, and their effect on the arm seemed now to diminish on standing, instead of increasing, as it did before; as the arm was drawn aside about $2\frac{1}{2}$ divisions less, at the end of an hour after the motion of the weights, than it was at first.

It seems sufficiently proved, therefore, that the effect in question is produced, as above explained, by the difference of temperature between the weights and case; for, in the 6th, 8th, and 9th experiments, in which the weights were not much warmer than the case, their effect increased but little on standing; whereas, it increased much, when they were much warmer than the case, and decreased much, when they were much cooler.

It must be observed, that in this apparatus, the box in which the balls play is pretty deep, and the balls hang near the bottom of it, which makes the effect of the current of air more sensible than it would otherwise be, and is a defect which I intend to rectify in some future experiments.

EXPERIMENT IX. April 29.

Weights in positive position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of middle of vibration.
		h ′ ″		h ′ ″
34,7 35 34,65	-	- -	34,84	

Weights moved to negative position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
23,8					
	28	11 18 29	-	11 18 43	
	29	58			
33,2	-	- -	28,52		
	29	25 27	-	25 40	
	28	57			
23,9	-	- -	28,25		
32	-	- -	28,01		
24,15	-	- -	27,82		
31	-	- -	27,63		
24,4	-	- -	27,55		
30,4	-	- -	27,47		
	28	0 7 4	-	0 7 26	
	27	53			
24,7					

Motion of arm - = 6,32
Time of vibration - = 6′ 58″

Experiment x. May 5.

Weights in positive position.

34,5					
33,5	-	- -	33,97		
34,4					

Weights moved to negative position.

22,3					
	28	10 43 42	-	10 43 36	
	29	44 6			
33,2	-	- -	27,82	-	7 0
	28	50 33	-	50 36	
	27	51 0			
22,6	-	- -	27,72		

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.	Difference.
		h. ′ ″		h. ′ ″	′ ″
32,5	-	- -	27,7		
23,2	-	- -	27,58		
31,45	-	- -	27,4		
23,5	-	- -	27,28		
	27	11 25 20	-	11 25 24	
	28	58			
30,7	-	- -	27,21	- -	7 3
	28	32 0	-	32 27	
	27	32 40			
23,95	-	- -	27,21	- -	6 56
	27	39 19	-	39 23	
	28	40 2			
30,25					

Motion of arm - = 6,15
Time of vibration - = 6′ 59″

EXPERIMENT XI. May 6.

Weights in positive position.

34,9					
34,1	-	- -	34,47		
34,8	-	- -	34,49		
34,25					

Weights moved to negative position.

23,3					
	28	9 59 59	-	10 0 8	
	29	10 0 27			
33,3	-	- -	28,42	7 5	
	29	6 52			
	27	7 51			

Extreme points.	Divisions.	Time.			Point of rest.	Time of mid. of vibration.		
		h.	'	"		h.	'	"
23,8	-		-	-	28,35			
32,5	-		-	-	28,3			
24,4 missed.								
24,8								
31,3	-		-	-	28,17			
	29	10	48	37	-	10	49	8
	28		49	21				
25,3	-		-	-	28,2			
	28		56	8	-		56	13
	29		56	56				
30,9								

Motion of arm - = 6,07
Time of vibration - = 7' 1"

In the three foregoing experiments, the index was purposely moved so that, before the beginning of the experiment, the balls rested as near the sides of the case as they could, without danger of touching it; for it must be observed, that when the arm is at 35, they begin to touch. In the two following experiments, the index was in its usual position.

EXPERIMENT XII. May 9.

Weights in negative position.

	17,4	9	45	0		
	17,4		58	0		
	17,4	10	8	0		
	17,4		10	0	17,4	

Weights moved to positive position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
		h. ′ ″		h. ′ ″
28,85				
	24	10 20 50	-	10 20 59
	22	21 46		
18,4	-	- -	23,49	
28,3	-	- -	23,57	
19,3	-	- -	23,67	
27,8	-	- -	23,72	
20	-	- -	23,8	
27,4	-	- -	23,83	
	24	11 3 13	-	11 3 14
	23	54		
20,55	-	- -	23,87	
	23	9 45	-	10 18
	24	10 28		
27				

Motion of arm - - - = 6,09
Time of vibration - - = 7′ 3″

EXPERIMENT XIII. May 25.

Weights in negative position.

16				
18,3	-	- -	17,2	
16,2				

Weights moved to positive position.

29,6				
	25	10 22 22	-	10 22 56
	24	0 45		
17,4	-	- -	23,32	
	23	29 59	-	30 3
	24	30 23		
28,9	-	- -	23,4	
	24	36 58	-	37 7
	23	37 24		
18,4	-	- -	23,52	

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
		h. ' "		h. ' "
	23	10 44 3	-	10 44 14
	24	31		
28,4	-	- -	23,62	
19,3	-	- -	23,7	
27,8	-	- -	23,7	
	24	11 5 26	-	11 5 31
	23	6 1		
19,9	-	- -	23,72	
	23	12 12	-	12 35
	24	50		
27,3				

Weights moved to negative position.

13,5				
21,8	-	- -	17,75	
	18	37 34	-	37 39
	17	38 10		
13,9	-	- -	17,67	
	17	44 26	-	44 45
	18	45 4		
21,1	-	- -	17,62	
14,4	-	- -	17,6	
20,5	-	- -	17,52	
14,7	-	- -	17,47	
20	-	- -	17,42	
	18	0 19 57	-	0 20 24
	17	20 52		
15	-	- -	17,37	
	17	27 15	-	27 30
	18	28 15		
19,5				

Motion of the arm on moving weights from — to + = 6,12
+ to — = 5,97

Time of vibration at - - - - + = 7′ 6″
— = 7 7

EXPERIMENT XIV. May 26.

Weights in negative position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
		h. ′ ″		h. ′ ″
	16,1	9 18 0		
	16,1	24 0		
	16,1	46 0		
	16,1	49 0	16,1	

Weights moved to positive position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
27,7				
	23	10 0 46	-	10 1 1
	22	1 16		
17,3	-	- -	22,37	
	22	7 58	-	8 5
	23	8 27		
27,2	-	- -	22,5	
	23	15 2	-	15 9
	22	32		
18,3	-	- -	22,65	
26,8	-	- -	22,75	
19,1	-	- -	22,85	
26,4	-	- -	22,97	
	23	43 40	-	43 32
	22	44 22		
20	-	- -	23,15	
	22	49 53	-	50 41
	23	50 37		
26,2				

Weights moved to negative position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
12,4				
	16	11 7 53	-	11 8 25
	17	8 27		
21,5	-	- -	17,02	
	17	15 30	-	15 27
	16	16 3		

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
		h. ′ ″		h. ′ ″
12,7	-	- -	16,9	
20,7	-	- -	16,85	
13,3	-	- -	16,82	
20	-	- -	16,72	
13,6	-	- -	16,67	
	16	11 50 33	-	11 50 58
	17	51 19		
19,5	-	- -	16,65	
	17	57 53	-	58 6
	16	58 44		
14				

Motion of arm by moving weights from — to + = 6,27
+ to — = 6,13
Time of vibration at + = 7′ 6″
— = 7 6

In the next experiment, the balls, before the motion of the weights, were made to rest as near as possible to the sides of the case, but on the contrary side from what they did in the 9th, 10th, and 11th experiments.

EXPERIMENT XV. May 27.

Weights in negative position.

3,9				
3,35	-	- -	3,61	
3,85	-	- -	3,61	
3,4				

Weights moved to positive position.

Extreme points.	Divisions.	Time. h.	′	″	Point of rest.	Time of mid. of vibration. h.	′	″
15,4								
	10	10	5	59	-	10	5	56
	9		6	27				
4,8	-		-	-	9,95			
	9		12	43	-		13	5
	10		13	11				
14,8	-		-	-	10,07			
	10		20	24	-		20	13
	9			56				
5,9	-		-	-	10,23			
14,35	-		-	-	10,35			
6,8	-		-	-	10,46			
13,9	-		-	-	10,52			
	11		48	30	-		48	42
	10		49	11				
7,5	-		-	-	10,6			
	10		55	26	-		55	48
	11		56	10				
13,5								

Motion of the arm - - - 6,34

Time of vibration - - 7′ 7″

The two following experiments were made by Mr. GILPIN, who was so good as to assist me on the occasion.

EXPERIMENT XVI. May 28.

Weights in negative position.

22,55				
8,4	-	-	-	15,09
21	-	-	-	14,9
9,2				

Weights moved to positive position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
		h. ′ ″		h. ′ ″
26,6				
	22	10 22 53	-	10 23 15
	21	23 20		
15,8	-	- -	21	
	20	30 7	-	30 30
	21	36		
25,8	-	- -	21,05	
	22	37 23	-	37 45
	21	55		
16,8	-	- -	21,11	
	20	44 29	-	45 1
	21	45 4		
25,05	-	- -	21,11	
	22	51 54	-	52 20
	21	52 32		
17,57	-	- -	21,2	
	21	59 31	-	59 34
	22	11 0 13		
24,6	-	- -	21,28	
	22	6 24	-	11 6 49
	21	7 9		
18,3				

Motion of the arm - - - - = 6,1
Time of vibration - - - - - = 7′ 16″

EXPERIMENT XVII. May 30.

Weights in negative position.

17,2	10 19 0		
17,1	25 0		
17,07	29 0		
17,15	40 0		
17,45	49 0		
17,42	51 0		
17,42	11 1 0	17,42	

Weights moved to positive position.

Extreme points.	Divisions.	Time.			Point of rest.	Time of mid. of vibration.		
		h.	′	″		h.	′	″
28,8								
	24	11	11	23	-	11	11	37
	23			49				
18,1	-		-	-	23,2			
	22		18	13	-		18	42
	23			43				
27,8	-		-	-	23,12			
	24		25	19	-		25	40
	23			49				
18,8	-		-	-	23,2			
	23		32	41	-		32	43
	24		33	13				
27,38	-		-	-	23,31			
	24		39	28	-		39	44
	23		40	3				
19,7	-		-	-	23,44			
	23		46	33	-		46	46
	24		47	11				
27	-		-	-	23,52			
	24		53	36	-		53	48
	23		54	17				
20,4	-		-	-	23,57			
	23	0	0	34	-	0	0	55
	24		1	18				
26,5	-		-	-	23,55			
	24		7	34	-		7	50
	23		8	21				
20,8	-		-	-	23,59			
	23		14	30	-		14	53
	24		15	24				
26,25								

Weights moved to negative position.

Extreme points.	Divisions.	Time.	Point of rest.	Time of mid. of vibration.
		h. ′ ″		h. ′ ″
13,3				
	17 18	0 32 19 48	-	0 32 44
22,4	-	- -	17,95	
	18 17	39 46 40 19	-	39 44
13,7	-	- -	17,85	
	17 18	46 26 47 0	-	46 48
21,6	-	- -	17,72	
	18 17	53 43 54 20	-	53 50
14	-	- -	17,6	
	17 18	1 0 39 1 20	-	1 0 55
20,8	-	- -	17,47	
	18 17	7 39 8 21	-	7 59
14,3	-	- -	17,37	
	17 18	14 54 15 42	-	15 4
20,1	-	- -	17,27	
	18 17	21 32 22 22	-	22 5
14,6				

Motion of the arm on moving weights from $- \text{ to } + = 5,78$
$+ \text{ to } - = 5,64$

Time of vibration at - - $+ = 7'\ 2''$
$- = 7\ 3$

On the Method of computing the Density of the Earth from these Experiments.

I shall first compute this, on the supposition that the arm and copper rods have no weight, and that the weights exert no sensible attraction, except on the nearest ball; and shall then examine what corrections are necessary, on account of the arm and rods, and some other small causes.

The first thing is, to find the force required to draw the arm aside, which, as was before said, is to be determined by the time of a vibration.

The distance of the centres of the two balls from each other is 73,3 inches, and therefore the distance of each from the centre of motion is 36,65, and the length of a pendulum vibrating seconds, in this climate, is 39,14; therefore, if the stiffness of the wire by which the arm is suspended is such, that the force which must be applied to each ball, in order to draw the arm aside by the angle A, is to the weight of that ball as the arch of A to the radius, the arm will vibrate in the same time as a pendulum whose length is 36,65 inches, that is, in $\sqrt{\frac{36,65}{39,14}}$ seconds; and therefore, if the stiffness of the wire is such as to make it vibrate in N seconds, the force which must be applied to each ball, in order to draw it aside by the angle A, is to the weight of the ball as the arch of $A \times \frac{1}{N^2} \times \frac{36,65}{39,14}$ to the radius. But the ivory scale at the end of the arm is 38,3 inches from the centre of motion, and each division is $\frac{1}{20}$ of an inch, and therefore subtends an angle at the centre, whose arch is $\frac{1}{766}$; and therefore the force which must be applied to each ball, to draw the

arm aside by one division, is to the weight of the ball as $\frac{1}{766\ N^2}\ \frac{36,65}{39,14}$ to 1, or as $\frac{1}{818\ N^2}$ to 1.

The next thing is, to find the proportion which the attraction of the weight on the ball bears to that of the earth thereon, supposing the ball to be placed in the middle of the case, that is, to be not nearer to one side than the other. When the weights are approached to the balls, their centres are 8,85 inches from the middle line of the case; but, through inadvertence, the distance, from each other, of the rods which support these weights, was made equal to the distance of the centres of the balls from each other, whereas it ought to have been somewhat greater. In consequence of this, the centres of the weights are not exactly opposite to those of the balls, when they are approached together; and the effect of the weights, in drawing the arm aside, is less than it would otherwise have been, in the triplicate ratio of $\frac{8,85}{36,65}$ to the chord of the angle whose sine is $\frac{8,85}{36,65}$, or in the triplicate ratio of the cosine of $\frac{1}{2}$ this angle to the radius, or in the ratio of ,9779 to 1.

Each of the weights weighs 2439000 grains, and therefore is equal in weight to 10,64 spherical feet of water; and therefore its attraction on a particle placed at the centre of the ball, is to the attraction of a spherical foot of water on an equal particle placed on its surface, as $10,64 \times ,9779 \times \overline{\frac{6}{8,85}}\big|^2$ to 1. The mean diameter of the earth is 41800000 feet;* and therefore, if the mean density of the earth is to that of water as D to one, the attraction of the leaden weight on the ball will be to that

* In strictness, we ought, instead of the mean diameter of the earth, to take the diameter of that sphere whose attraction is equal to the force of gravity in this climate; but the difference is not worth regarding.

of the earth thereon, as $10,64 \times ,9779 \times \overline{\frac{6}{8,85}}\Big|^2$ to 41800000 D :: 1 to 8739000 D.

It is shewn, therefore, that the force which must be applied to each ball, in order to draw the arm one division out of its natural position, is $\frac{1}{818\,N^2}$ of the weight of the ball; and, if the mean density of the earth is to that of water as D to 1, the attraction of the weight on the ball is $\frac{1}{8739000\,D}$ of the weight of that ball; and therefore the attraction will be able to draw the arm out of its natural position by $\frac{818\,N^2}{8739000\,D}$ or $\frac{N^2}{10683\,D}$ divisions; and therefore, if on moving the weights from the midway to a near position the arm is found to move B divisions, or if it moves 2 B divisions on moving the weights from one near position to the other, it follows that the density of the earth, or D, is $\frac{N^2}{10683\,B}$.

We must now consider the corrections which must be applied to this result; first, for the effect which the resistance of the arm to motion has on the time of the vibration: 2d, for the attraction of the weights on the arm: 3d, for their attraction on the farther ball: 4th, for the attraction of the copper rods on the balls and arm: 5th, for the attraction of the case on the balls and arm: and 6th, for the alteration of the attraction of the weights on the balls, according to the position of the arm, and the effect which that has on the time of vibration. None of these corrections, indeed, except the last, are of much signification, but they ought not entirely to be neglected.

As to the first, it must be considered, that during the vibrations of the arm and balls, part of the force is spent in accele-

rating the arm; and therefore, in order to find the force required to draw them out of their natural position, we must find the proportion which the forces spent in accelerating the arm and balls bear to each other.

Let EDC *e d c* (fig. 4.) be the arm. B and *b* the balls. C *s* the suspending wire. The arm consists of 4 parts; first, a deal rod D *c d*, 73,3 inches long; 2d, the silver wire DC *d*, weighing 170 grains; 3d, the end pieces DE and *e d*, to which the ivory vernier is fastened, each of which weighs 45 grains; and 4th, some brass work C *c*, at the centre. The deal rod, when dry, weighs 2320 grains, but when very damp, as it commonly was during the experiments, weighs 2400; the transverse section is of the shape represented in fig. 5; the thickness BA, and the dimensions of the part DE *ed*, being the same in all parts; but the breadth B *b* diminishes gradually, from the middle to the ends. The area of this section is ,33 of a square inch at the middle, and ,146 at the end; and therefore, if any point x (fig. 4.) is taken in $c\,d$, and $\frac{c\,x}{c\,d}$ is called x, this rod weighs $\frac{2400 \times ,33}{73,3 \times ,238}$ per inch at the middle; $\frac{2400 \times ,146}{73,3 \times ,238}$ at the end, and $\frac{2400}{73,3} \times \frac{,33 - ,184\,x}{,238}$ $= \frac{3320 - 1848\,x}{73,3}$ at x; and therefore, as the weight of the wire is $\frac{170}{73,3}$ per inch, the deal rod and wire together may be considered as a rod whose weight at $x = \frac{3490 - 1848\,x}{73,3}$ per inch.

But the force required to accelerate any quantity of matter placed at x, is proportional to x^2; that is, it is to the force required to accelerate the same quantity of matter placed at d as x^2 to 1; and therefore, if $c\,d$ is called l, and x is supposed to flow, the fluxion of the force required to accelerate the deal rod

and wire is proportional to $\frac{x^2 l \dot{x} \times 3490 - 1848 x}{73{,}3}$, the fluent of which, generated while x flows from c to d, $= \frac{l}{73{\cdot}3} \times \frac{3490}{3} - \frac{1848}{4} = 350$; so that the force required to accelerate each half of the deal rod and wire, is the same as is required to accelerate 350 grains placed at d.

The resistance to motion of each of the pieces $d e$, is equal to that of 48 grains placed at d; as the distance of their centres of gravity from C is 38 inches. The resistance of the brass work at the centre may be disregarded; and therefore the whole force required to accelerate the arm, is the same as that required to accelerate 398 grains placed at each of the points D and d.

Each of the balls weighs 11262 grains, and they are placed at the same distance from the centre as D and d; and therefore, the force required to accelerate the balls and arm together, is the same as if each ball weighed 11660, and the arm had no weight; and therefore, supposing the time of a vibration to be given, the force required to draw the arm aside, is greater than if the arm had no weight, in the proportion of 11660 to 11262, or of 1,0353 to 1.

To find the attraction of the weights on the arm, through d draw the vertical plane $d w b$ perpendicular to D d, and let w be the centre of the weight, which, though not accurately in this plane, may, without sensible error, be considered as placed therein, and let b be the centre of the ball; then $w b$ is horizontal and $= 8{,}85$, and $d b$ is vertical and $= 5{,}5$; let $w a = a$, $w b = b$, and let $\frac{d x}{d c}$, or $1 - x = z$; then the attraction of the weight on a particle of matter at x, in the direction $d w$, is to its attraction on the same particle placed at b : : $b^3 : \overline{a^2 + z^2 l^2}^{\frac{3}{2}}$, or is pro-

portional to $\frac{b^3}{\overline{a^2+z^2l^2}|^{\frac{3}{2}}}$, and the force of that attraction to move the arm, is proportional to $\frac{b^3\times\overline{1-z}}{\overline{a^2+z^2l^2}|^{\frac{3}{2}}}$, and the weight of the deal rod and wire at the point x, was before said to be $\frac{3490-1848x}{73,3}=\frac{1642+1848z}{73,3}$ per inch; and therefore, if dx flows, the fluxion of the power to move the arm $=l\dot{z}\times\frac{1642+1848z}{73,3}$ $+\frac{b^3\times\overline{1-z}}{\overline{a^2+z^2l^2}|^{\frac{3}{2}}}=\dot{z}\times\overline{821+924z}\times\frac{b^3\times\overline{1-z}}{\overline{a^2+l^2z^2}|^{\frac{3}{2}}}=\frac{b^3\dot{z}\times\overline{821+103z-924z^2}}{\overline{a^2+l^2z^2}|^{\frac{3}{2}}}$ $=\frac{b^3z\times\overline{821+103z+\frac{924a^2}{l^2}}}{\overline{a^2+l^2z^2}|^{\frac{3}{2}}}-\frac{924b^3\dot{z}\times\overline{\frac{a^2}{l^2}+z^2}}{\overline{a^2+l^2z^2}|^{\frac{3}{2}}}$; which, as $\frac{a^2}{l^2}=,08=$ $\frac{b^3\dot{z}\times\overline{895+103z}}{\overline{a^2+l^2z^2}|^{\frac{3}{2}}}-\frac{924b^3\dot{z}}{l^2\sqrt{a^2+l^2z^2}}$. The fluent of this $=\frac{895b^3z}{a^2\sqrt{a^2+l^2z^2}}$ $-\frac{103b^3}{l^2\sqrt{a^2+l^2z^2}}+\frac{103b^3}{l^2a}-\frac{924b^3}{l^3}\log.\frac{lz+\sqrt{a^2+l^2z^2}}{a}$, and the force with which the attraction of the weight, on the nearest half of the deal rod and wire, tends to move the arm, is proportional to this fluent generated while z flows from 0 to 1, that is, to 128 grains.

The force with which the attraction of the weight on the end-piece *de* tends to move the arm, is proportional to $47\times\frac{b^3}{a^3}$, or 29 grains; and therefore the whole power of the weight to move the arm, by means of its attraction on the nearest part thereof, is equal to its attraction on 157 grains placed at *b*, which is $\frac{157}{11260}$, or ,0139 of its attraction on the ball.

It must be observed, that the effect of the attraction of the weight on the whole arm is rather less than this, as its attraction on the farther half draws it the contrary way; but, as the attraction on this is small, in comparison of its attraction on the nearer half, it may be disregarded.

The attraction of the weight on the furthest ball, in the direction bw, is to its attraction on the nearest ball : : $wd^3 : wD^3$: : ,0017 : 1; and therefore the effect of the attraction of the weight on both balls, is to that of its attraction on the nearest ball : : ,9983 : 1.

To find the attraction of the copper rod on the nearest ball, let b and w (fig. 6.) be the centres of the ball and weight, and ea the perpendicular part of the copper rod, which consists of two parts, ad and de. ad weighs 22000 grains, and is 16 inches long, and is nearly bisected by w. de weighs 41000, and is 46 inches long. wb is 8,85 inches, and is perpendicular to ew. Now, the attraction of a line ew, of uniform thickness, on b, in the direction bw, is to that of the same quantity of matter placed at w : : $bw : eb$; and therefore the attraction of the part da equals that of $\frac{22000 \times wb}{db}$, or 16300, placed at w; and the attraction of de equals that of $41000 \times \frac{ew}{ed} \times \frac{bw}{be} - 41000 \times \frac{dw}{ed} \times \frac{bw}{bd}$, or 2500, placed at the same point; so that the attraction of the perpendicular part of the copper rod on b, is to that of the weight thereon, as 18800 : 2439000, or as ,00771 to 1. As for the attraction of the inclined part of the rod and wooden bar, marked Pr and rr in fig. 1, it may safely be neglected, and so may the attraction of the whole rod on the arm and farthest ball; and therefore the attraction of the weight and copper rod, on the arm and both balls together, exceeds the attraction of the weight on the nearest ball, in the proportion of ,9983 + ,0139 + ,0077 to one, or of 1,0199 to 1.

The next thing to be considered, is the attraction of the mahogany case. Now it is evident, that when the arm stands at

the middle division, the attractions of the opposite sides of the case balance each other, and have no power to draw the arm either way. When the arm is removed from this division, it is attracted a little towards the nearest side, so that the force required to draw the arm aside is rather less than it would otherwise be; but yet, if this force is proportional to the distance of the arm from the middle division, it makes no error in the result; for, though the attraction will draw the arm aside more than it would otherwise do, yet, as the accelerating force by which the arm is made to vibrate is diminished in the same proportion, the square of the time of a vibration will be increased in the same proportion as the space by which the arm is drawn aside, and therefore the result will be the same as if the case exerted no attraction; but, if the attraction of the case is not proportional to the distance of the arm from the middle point, the ratio in which the accelerating force is diminished is different in different parts of the vibration, and the square of the time of a vibration will not be increased in the same proportion as the quantity by which the arm is drawn aside, and therefore the result will be altered thereby.

On computation, I find that the force by which the attraction draws the arm from the centre is far from being proportional to the distance, but the whole force is so small as not to be worth regarding; for, in no position of the arm does the attraction of the case on the balls exceed that of $\frac{1}{5}$th of a spheric inch of water, placed at the distance of 1 inch from the centre of the balls; and the attraction of the leaden weight equals that of 10,6 spheric feet of water placed at 8,85 inches, or of 234 spheric inches placed at 1 inch distance; so that the attraction of the case on the balls can in no position of the arm exceed

$\frac{1}{1170}$ of that of the weight. The computation is given in the Appendix.

It has been shewn, therefore, that the force required to draw the arm aside one division, is greater than it would be if the arm had no weight, in the ratio of 1,0353 to 1, and therefore $= \frac{1,0353}{818\ N^2}$ of the weight of the ball; and moreover, the attraction of the weight and copper rod on the arm and both balls together, exceeds the attraction of the weight on the nearest ball, in the ratio of 1,0199 to 1, and therefore $= \frac{1,0199}{8739000\ D}$ of the weight of the ball; consequently D is really equal to $\frac{818\ N^2}{1,0353} \times \frac{1,0199}{8739000\ B}$, or $\frac{N^2}{10844\ B}$, instead of $\frac{N^2}{10683\ B}$, as by the former computation. It remains to be considered how much this is affected by the position of the arm.

Suppose the weights to be approached to the balls; let W, (fig. 7.) be the centre of one of the weights; let M be the centre of the nearest ball at its mean position, as when the arm is at 20 divisions; let B be the point which it actually rests at; and let A be the point which it would rest at, if the weight was removed; consequently, AB is the space by which it is drawn aside by means of the attraction; and let $M\beta$ be the space by which it would be drawn aside, if the attraction on it was the same as when it is at M. But the attraction at B is greater than at M, in the proportion of $WM^2 : WB^2$; and therefore, $AB = M\beta \times \frac{WM^2}{WB^2} = M\beta \times 1 + \frac{2MB}{MW}$, very nearly.

Let now the weights be moved to the contrary near position, and let w be now the centre of the nearest weight, and b the point of rest of the centre of the ball; then $Ab = M\beta \times 1 + \frac{2Mb}{MW}$, and $Bb = M\beta \times 2 + \frac{2Mb}{MW} + \frac{2MB}{MW} = 2M\beta \times 1 + \frac{Bb}{MW}$; so

that the whole motion Bb is greater than it would be if the attraction on the ball was the same in all places as it is at M, in the ratio of $1 + \frac{Bb}{MW}$ to one; and, therefore, does not depend sensibly on the place of the arm, in either position of the weights, but only on the quantity of its motion, by moving them.

This variation in the attraction of the weight, affects also the time of vibration; for, suppose the weights to be approached to the balls, let W be the centre of the nearest weight; let B and A represent the same things as before; and let x be the centre of the ball, at any point of its vibration; let AB represent the force with which the ball, when placed at B, is drawn towards A by the stiffness of the wire; then, as B is the point of rest, the attraction of the weight thereon will also equal AB; and, when the ball is at x, the force with which it is drawn towards A, by the stiffness of the wire, $= Ax$, and that with which it is drawn in the contrary direction, by the attraction, $= AB \times \frac{WB^2}{Wx^2}$; so that the actual force by which it is drawn towards $A = Ax - \frac{AB \times WB^2}{Wx^2} = AB + Bx - \overline{AB \times 1 + \frac{2Bx}{WB}} = Bx - \frac{2Bx \times AB}{WB}$, very nearly. So that the actual force with which the ball is drawn towards the middle point of the vibration, is less than it would be if the weights were removed, in the ratio of $1 - \frac{2AB}{WB}$ to one, and the square of the time of a vibration is increased in the ratio of 1 to $1 - \frac{2AB}{WB}$; which differs very little from that of $1 + \frac{Bb}{MW}$ to 1, which is the ratio in which the motion of the arm, by moving the weights from one near position to the other, is increased.

The motion of the ball answering to one division of the arm $= \frac{36,35}{20 \times 38,3}$; and, if mB is the motion of the ball answering to d divisions on the arm, $\frac{MB}{WM} = \frac{36,35\,d}{20 \times 38,3 \times 8,85} = \frac{d}{185}$; and therefore, the time of vibration, and motion of the arm, must be corrected as follows:

If the time of vibration is determined by an experiment in which the weights are in the near position, and the motion of the arm, by moving the weights from the near to the midway position, is d divisions, the observed time must be diminished in the subduplicate ratio of $1 - \frac{2d}{185}$ to 1, that is, in the ratio of $1 - \frac{d}{185}$ to 1; but, when it is determined by an experiment in which the weights are in the midway position, no correction must be applied.

To correct the motion of the arm caused by moving the weights from a near to the midway position, or the reverse, observe how much the position of the arm differs from 20 divisions, when the weights are in the near position: let this be n divisions, then, if the arm at that time is on the same side of the division of 20 as the weight, the observed motion must be diminished by the $\frac{2n}{185}$ part of the whole; but, otherwise, it must be as much increased.

If the weights are moved from one near position to the other, and the motion of the arm is $2d$ divisions, the observed motion must be diminished by the $\frac{2d}{185}$ part of the whole.

If the weights are moved from one near position to the other, and the time of vibration is determined while the weights are in one of those positions, there is no need of correcting either the motion of the arm, or the time of vibration.

CONCLUSION.

The following Table contains the Result of the Experiments.

Exper.	Mot. weight	Mot. arm	Do. corr.	Time vib.	Do. corr.	Density.
1	m. to +	14,32	13,42	′ ″	-	5,5
	+ to m.	14,1	13,17	14,55	-	5,61
2	m. to +	15,87	14,69	-	-	4,88
	+ to m.	15,45	14,14	14,42	-	5,07
3	+ to m.	15,22	13,56	14,39	-	5,26
	m. to +	14,5	13,28	14,54	-	5,55
4	m. to +	3,1	2,95		6,54	5,36
	+ to −	6,18	-	7,1	-	5,29
	− to +	5,92	-	7,3	-	5,58
5	+ to −	5,9	-	7,5	-	5,65
	− to +	5,98	-	7,5	-	5,57
6	m. to −	3,03	2,9	-	-	5,53
	− to +	5,9	5,71		-	5,62
7	m. to −	3,15	3,03	7,4 by mean.	6,57	5,29
	− to +	6,1	5,9			5,44
8	m. to −	3,13	3,00		-	5,34
	− to +	5,72	5,54	-	-	5,79
9	+ to −	6,32	-	6,58	-	5,1
10	+ to −	6,15	-	6,59	-	5,27
11	+ to −	6,07	-	7,1	-	5,39
12	− to +	6,09	-	7,3	-	5,42
13	− to +	6,12	-	7,6	-	5,47
	+ to −	5,97	-	7,7	-	5,63
14	− to +	6,27	-	7,6	-	5,34
	+ to −	6,13	-	7,6	-	5,46
15	− to +	6,34	-	7,7	-	5,3
16	− to +	6,1	-	7,16	-	5,75
17	− to +	5,78	-	7,2	-	5,68
	+ to −	5,64	-	7,3	-	5,85

From this table it appears, that though the experiments agree pretty well together, yet the difference between them, both in the quantity of motion of the arm and in the time of vibration, is greater than can proceed merely from the error of observation. As to the difference in the motion of the arm, it may very well be accounted for, from the current of air produced by the difference of temperature; but, whether this can account for the difference in the time of vibration, is doubtful. If the current of air was regular, and of the same swiftness in all parts of the vibration of the ball, I think it could not; but, as there will most likely be much irregularity in the current, it may very likely be sufficient to account for the difference.

By a mean of the experiments made with the wire first used, the density of the earth comes out 5,48 times greater than that of water; and by a mean of those made with the latter wire, it comes out the same; and the extreme difference of the results of the 23 observations made with this wire, is only ,75; so that the extreme results do not differ from the mean by more than ,38, or $\frac{1}{14}$ of the whole, and therefore the density should seem to be determined hereby, to great exactness. It, indeed, may be objected, that as the result appears to be influenced by the current of air, or some other cause, the laws of which we are not well acquainted with, this cause may perhaps act always, or commonly, in the same direction, and thereby make a considerable error in the result. But yet, as the experiments were tried in various weathers, and with considerable variety in the difference of temperature of the weights and air, and with the arm resting at different distances from the sides of the case, it seems very unlikely that this cause should act so uniformly in the same way, as to make the error of the mean result nearly

equal to the difference between this and the extreme; and, therefore, it seems very unlikely that the density of the earth should differ from 5,48 by so much as $\frac{1}{14}$ of the whole.

Another objection, perhaps, may be made to these experiments, namely, that it is uncertain whether, in these small distances, the force of gravity follows exactly the same law as in greater distances. There is no reason, however, to think that any irregularity of this kind takes place, until the bodies come within the action of what is called the attraction of cohesion, and which seems to extend only to very minute distances. With a view to see whether the result could be affected by this attraction, I made the 9th, 10th, 11th, and 15th experiments, in which the balls were made to rest as close to the sides of the case as they could; but there is no difference to be depended on, between the results under that circumstance, and when the balls are placed in any other part of the case.

According to the experiments made by Dr. MASKELYNE, on the attraction of the hill Schehallien, the density of the earth is $4\frac{1}{2}$ times that of water; which differs rather more from the preceding determination than I should have expected. But I forbear entering into any consideration of which determination is most to be depended on, till I have examined more carefully how much the preceding determination is affected by irregularities whose quantity I cannot measure.

APPENDIX.

On the Attraction of the Mahogany Case on the Balls.

THE first thing is, to find the attraction of the rectangular plane $c\,k\,\beta\,b$ (fig. 8.) on the point a, placed in the line $a\,c$ perpendicular to this plane.

Let $a\,c = a$, $c\,k = b$, $c\,b = x$, and let $\frac{a^2}{a^2+x^2} = w^2$, and $\frac{b^2}{a^2+x^2} = v^2$, then the attraction of the line $b\,\beta$ on a, in the direction $a\,b, = \frac{b\,\beta}{a\,b \times a\,\beta}$; and therefore, if $c\,b$ flows, the fluxion of the attraction of the plane on the point a, in the direction $c\,b$,

$$= \frac{b\,\dot{x}}{\sqrt{a^2+x^2} \times \sqrt{a^2+b^2+x^2}} \times \frac{x}{\sqrt{a^2+x^2}} = \frac{-b\,\dot{w}}{w\sqrt{b^2+\frac{a^2}{w^2}}} = \frac{-b\,\dot{w}}{\sqrt{b^2w^2+a^2}} = \frac{-\dot{v}}{\sqrt{1+v^2}},$$

the variable part of the fluent of which $= -\log.\ v + \sqrt{1+v^2}$, and therefore the whole attraction $= \log.\ \frac{c\,k+a\,k}{a\,c} \times \frac{a\,b}{b\,\beta+a\,\beta}$; so that the attraction of the plane, in the direction $c\,b$, is found readily by logarithms, but I know no way of finding its attraction in the direction $a\,c$, except by an infinite series.

The two most convenient series I know, are the following:

First series. Let $\frac{b}{a} = \pi$, and let A = arc whose tang. is π, $B = A - \pi$, $C = B + \frac{\pi^3}{3}$, $D = C - \frac{\pi^5}{5}$, &c. then the attraction in the direction $a\,c = \sqrt{1-w^2} \times A + \frac{B\,w^2}{2} + \frac{3C\,w^4}{2.4} + \frac{3.5\,w^6}{2.4.6}$, &c.

For the second series, let $A =$ arc whose tang. $= \frac{1}{\pi}$, $B = A - \frac{1}{\pi^3}$, $C = B + \frac{1}{3\pi 3}$, $D = C - \frac{1}{5\pi 5}$, &c. then the attraction $=$ arc . $90°$ $- \sqrt{1 + v^2} \times A - \frac{Bv^2}{2} + \frac{3Cv^4}{2.4} - \frac{3.5Dv^6}{2.4.6}$, &c.

It must be observed, that the first series fails when π is greater than unity, and the second, when it is less; but, if b is taken equal to the least of the two lines ck and cb, there is no case in which one or the other of them may not be used conveniently.

By the help of these series, I computed the following table.

	,1962	,3714	,5145	,6248	,7071	,7808	,8575	,9285	,9815	1,
,1962	,00001									
,3714	,00039	00148								
,5145	,00074	00277	00521							
,6248	00110	00406	00778	01183						
,7071	00140	00522	01008	01525	02002					
,7808	00171	00637	01245	01896	02405	03247				
,8575	00207	00772	01522	02339	03116	03964	05057			
,9285	00244	00910	01810	02807	03778	04867	06319	08119		
,9815	00271	01019	02084	03193	04368	05639	07478	09931	12849	
1.	00284	01054	02135	03347	04560	05975	07978	10789	14632	19612

Find in this table, with the argument $\frac{ck}{ak}$ at top, and the argument $\frac{cb}{ab}$ in the left hand column, the corresponding logarithm; then add together this logarithm, the logarithm of $\frac{ck}{ak}$, and the logarithm of $\frac{cb}{ab}$; the sum is logarithm of the attraction.

To compute from hence the attraction of the case on the ball, let the box DCBA, (fig. 1.) in which the ball plays, be divided into two parts, by a vertical section, perpendicular to the length of the case, and passing through the centre of the ball; and, in fig. 9, let the parallelopiped ABDE$abde$ be one of these parts, ABDE being the abovementioned vertical section; let x be the centre of the ball, and draw the parallelogram $\beta n p m \delta x$ parallel to BbdD, and $xgrp$ parallel to βBbn, and bisect $\beta\delta$ in c. Now, the dimensions of the box, on the inside, are Bb = 1,75; BD = 3,6; Bβ = 1,75; and βA = 5; whence I find, that if xc and βx are taken as in the two upper lines of the following table, the attractions of the different parts are as set down below.

xc - -	,75	,5	,25
βx - -	1,05	1,3	1,55
Excess of attract. of Ddrg above Bbrg	,2374	,1614	,0813
——— $mdrp$ above $nbrp$	,2374	,1614	,0813
——— $mesp$ above $nasp$	,3705	,2516	,1271
Sum of these -	,8453	,5744	,2897
Excess of attract. of B$bn\beta$ above D$dm\delta$	,5007	,3271	,1606
——— A$an\beta$ above E$em\delta$	,4677	,3079	,1525
Whole attraction of the inside surface of the half box - - -	,1231	,0606	,0234

It appears, therefore, that the attraction of the box on x increases faster than in proportion to the distance xc.

The specific gravity of the wood used in this case is ,61, and its thickness is $\frac{3}{4}$ of an inch; and therefore, if the attraction of the outside surface of the box was the same as that of the inside, the whole attraction of the box on the ball, when cx = ,75,

would be equal to $2 \times ,1231 \times ,61 \times \frac{3}{4}$ cubic inches, or ,201 spheric inches of water, placed at the distance of one inch from the centre of the ball. In reality, it can never be so great as this, as the attraction of the outside surface is rather less than that of the inside; and, moreover, the distance of x from c can never be quite so great as ,75 of an inch, as the greatest motion of the arm is only $1\frac{1}{2}$ inch.

Fig. 1.

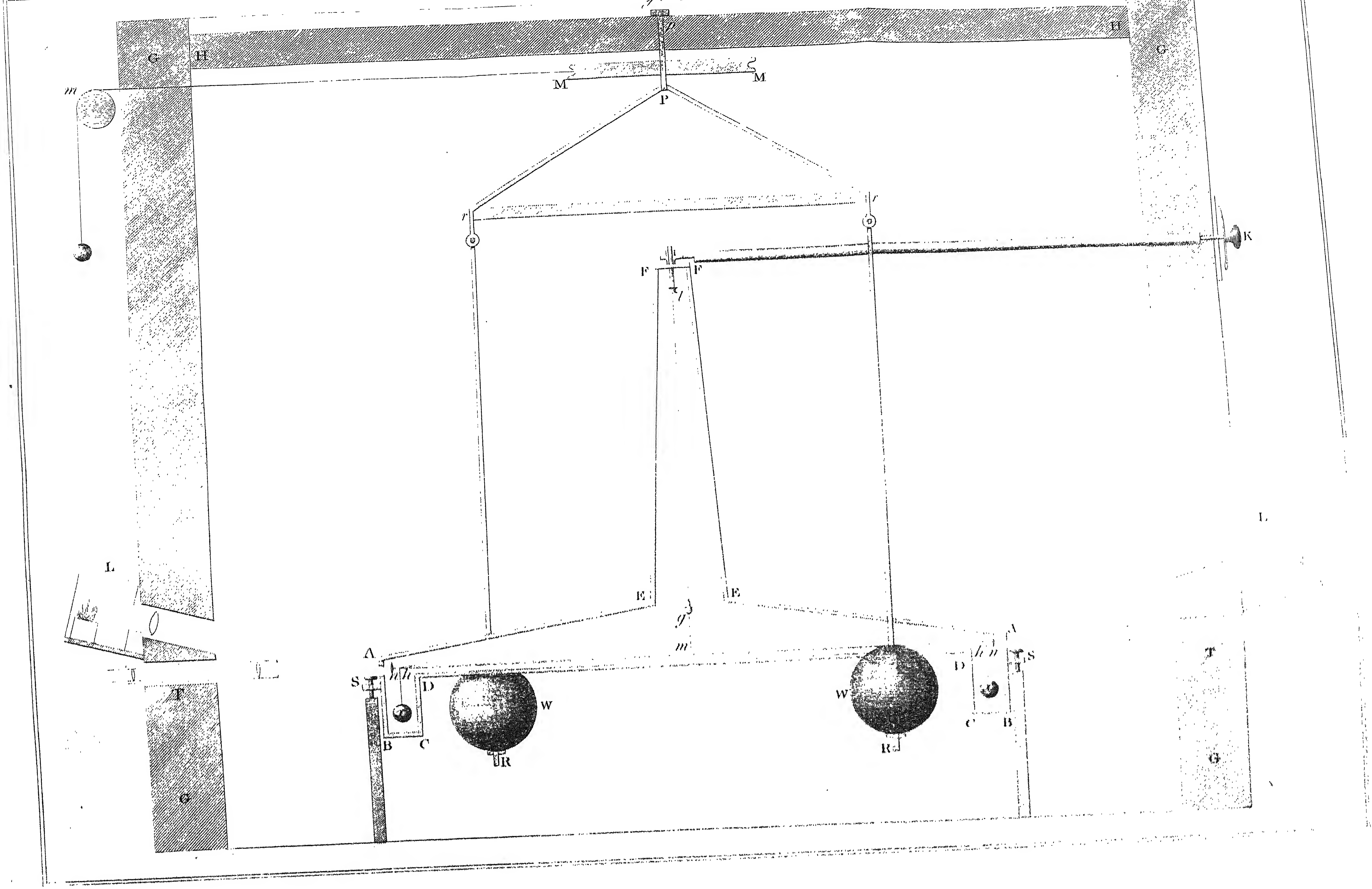


Fig. 2.

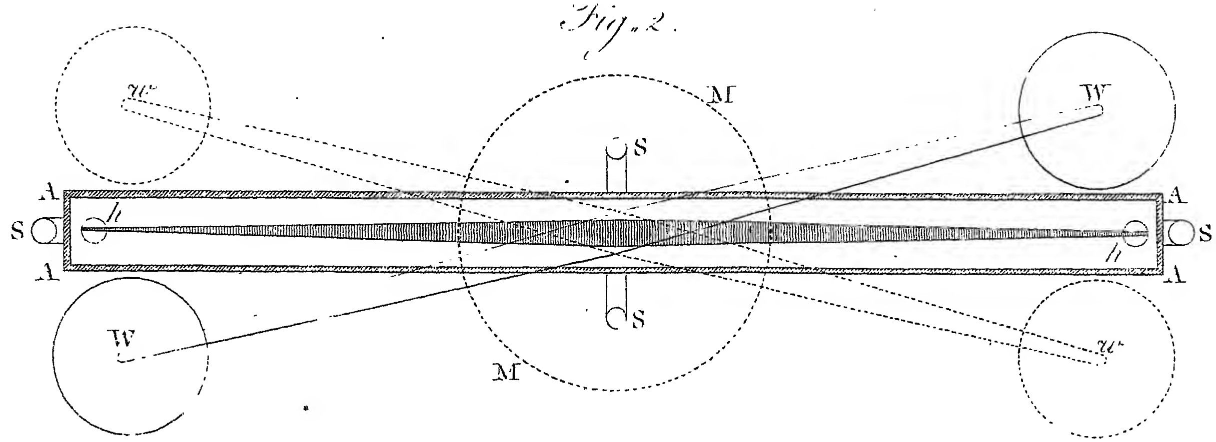


Fig. 3.

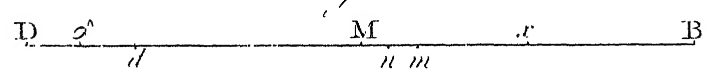


Fig. 4.

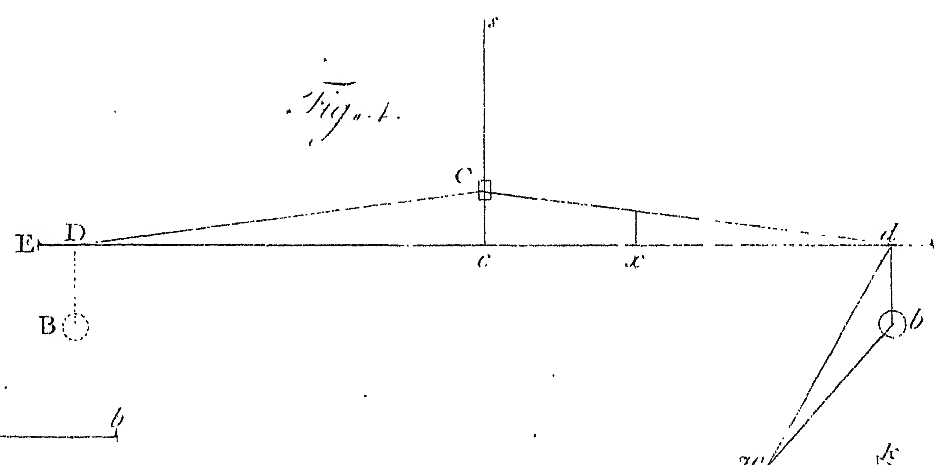


Fig. 5.

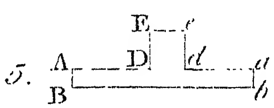


Fig. 6.

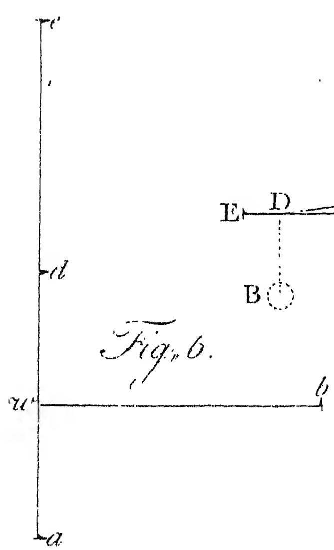


Fig. 7.

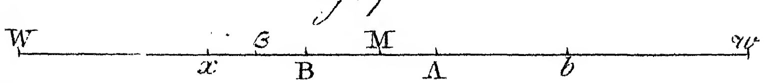


Fig. 8.

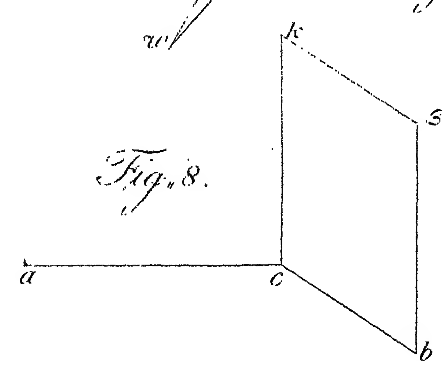
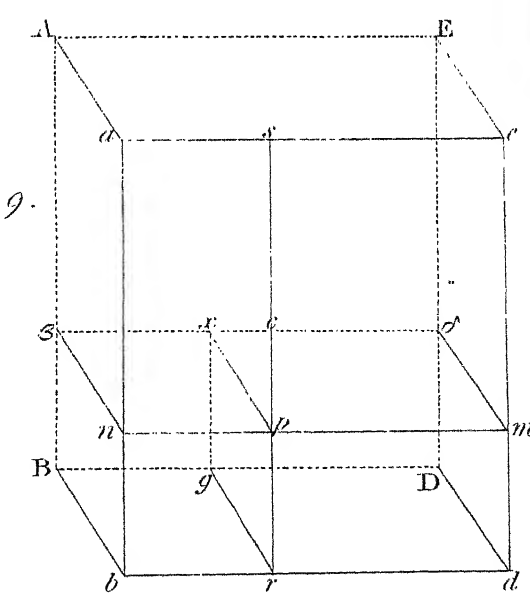


Fig. 9.



XXII. *An improved Solution of a Problem in physical Astronomy; by which swiftly converging Series are obtained, which are useful in computing the Perturbations of the Motions of the Earth, Mars, and Venus, by their mutual Attraction. To which is added an Appendix, containing an easy Method of obtaining the Sums of many slowly converging Series which arise in taking the Fluents of binomial Surds, &c. By the Rev.* John Hellins, *F. R. S. Vicar of Potter's Pury, in Northamptonshire. In a Letter to the Rev.* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read June 28, 1798.

REVEREND SIR, Potter's Pury, April 17, 1797.

Such is the subject of the inclosed paper, and such the reputation for skill and industry, which the many valuable papers you have communicated to the Royal Society, and your other learned works, have justly procured to you, that it could not with more propriety be submitted to the judgment of any other person than yourself, even if the writer of it were a stranger to you.

But there are circumstances which render my presenting it to you, in some measure, a duty. I had the advantage of being, for some years, your Assistant in the Royal Observatory at Greenwich; during which time, you made the important observations on the mountain *Schehallien*, in Scotland, which afford

an ocular demonstration of the attraction of that mountain, and a strong argument for the general attraction of matter, a subject nearly connected with that of the following pages; and it was from you that I received the problem of which you will here find an improved solution.

The diffidence with which I entered on a speculation which had engaged the attention of such learned men as SIMPSON, EULER, and DE LA GRANGE, is well known to you. Considering the great abilities of these men, and the length of time which EULER, in particular, appears to have employed on the subject, all that I at first expected to effect was, to facilitate the summation of the slowly converging series by means of which they had computed the perturbations of the motions of the planets in their orbits, which arise from their actions on one another, by the force of gravity; and that this might be done by a method which I had some time before discovered, was evident, on inspecting their series. Here, it is probable, I should have stopped, had not you been pleased to put into my hands a sheet of paper, written by the late Mr. SIMPSON, which, though very ingenious, was, by mistakes, which seem to have entered in transcribing it, rendered unintelligible to some eminent mathematicians who had perused it; in which state it had remained thirty-six years. On perusing this paper, the first thing that occurred to me was, a different method of finding the fluent, from that which had been used by Mr. SIMPSON; by which means, series converging by the powers of $\frac{1}{6}$ were obtained, while the series brought out the common way lost all convergency by a geometrical progression, and a computation by it was more difficult than the computation of the length of

a quadrantal arch of the circle by the series $1 + \frac{1}{2 \cdot 3} + \frac{3}{2 \cdot 4 \cdot 5} + \frac{3 \cdot 5}{2 \cdot 4 \cdot 6 \cdot 7}$, *&c.* Afterwards, I discovered the method of transforming that series which had lost all convergency by a geometrical progression, into another in which the literal powers decrease very swiftly; which is the improvement I now offer to you.

In comparing the series here produced, for computing the values of A and B in the equation $(a - b \times \cos. z)^{-n} = A + B \cdot \cos. z + C \cdot \cos. 2z + D \cdot \cos. 3z + \&c.$ with those which have been published for that purpose, by Messrs. EULER and DE LA GRANGE, it will appear, that those cases which were the most difficult to be computed by their methods, are the most easy by mine. For instance, if Venus's perturbation of the motion of the Earth were to be computed, (and *vice versâ,*) the literal powers which have place in M. EULER's series, would be very nearly equal to the powers of $\frac{9}{10}$; the literal powers which have place in M. DE LA GRANGE's series, would be nearly equal to the powers of $\frac{1}{2}$; and, in the series now produced, the literal powers would decrease somewhat swifter than the powers of $\frac{1}{36}$.

M. DE LA GRANGE has indeed, by a very ingenious device, obtained a convergency in the numeral coefficients of the series that he uses, which, for the first five terms of it, is nearly equal to the powers of $\frac{1}{4}$; but this convergency becomes less and less in every succeeding term, and the coefficients approach pretty fast to a ratio of equality; so that, to obtain the sum of the series to six places of decimals, he proposes to compute the first ten terms of it. The case in which those coefficients

have that convergency, is when n (which answers to his s,) is $= \frac{-1}{2}$, a case which does not often happen; however, from the values of A and B, when $n = \frac{-1}{2}$, he derives their values when $n = \frac{1}{2}, \frac{3}{2}$, *&c.* by another very ingenious device, worthy of that skill for which he is justly celebrated. But, by the method now proposed, the chief part of the convergency is in the literal powers; and such a difference in the numeral coefficients, for a different value of n, does not take place.

For Mars's perturbation of the Earth's motion, the literal powers by which the three different series converge, are nearly as follows:

$$\left.\begin{array}{l}\text{M. EULER's,}\\ \text{M. DE LA GRANGE's,}\\ \text{The series now proposed,}\end{array}\right\}\text{by the powers of}\left\{\begin{array}{l}\frac{4}{5};\\ \frac{10}{23};\\ \frac{1}{20}.^{*}\end{array}\right.$$

If, indeed, the perturbation which arises from the action of Jupiter upon the earth was to be computed, M. DE LA GRANGE's series would be the best that has hitherto been published for the purpose, as the literal powers of it would, in that case, be

* For obtaining nearly the different rates of convergency of the literal powers in the three series, it will be sufficient to consider the distance of the two planets of which the perturbations are to be computed, as $= \sqrt{(RR + rr - 2Rr \times c, z)}$, where R and r denote their mean distances from the sun, of which R is the greater, and c, z the cosine of the angle of commutation. Then will M. DE LA GRANGE's series converge by the powers of the quantity $\frac{rr}{RR}$; and, since $RR + rr = a$, and $2Rr = b$, in our notation, and the converging quantity in M. EULER's series is $(nn) = \frac{bb}{aa}$, it will be $= \frac{4R^2r^2}{(RR+rr)^2}$; and cc, by the powers of which the new series converges, is $= \frac{a-b}{a+b} = \frac{RR - 2Rr + rr}{RR + 2Rr + rr} = \frac{(R-r)^2}{(R+r)^2}$. See the *Memoirs of the Royal Academy of Sciences and Belles-Lettres* at Berlin, for 1781, p. 257; M. EULER's *Institutiones Calculi Integralis*, Vol. I. p. 186; and Art. 4, in what follows.

nearly equal to the powers of $\frac{1}{27}$, while the literal powers in the new series would differ but little from those of $\frac{11}{24}$. So that, for computing the perturbation of each of these three planets, we now have series converging so very swiftly, that the first four terms are sufficient for the purpose.

These indeed are the perturbations of motion, arising from the actions of the planets, which the inhabitants of this globe have most frequent occasion to compute. And, since two of the three are most easily calculated by the method explained in the following pages, I am not without hopes that I have rendered an acceptable piece of service to astronomers in general, and more especially to those who are most intent upon improving astronomical tables.

But it may be proper to remark, that the use of the new series is not confined to the computations just mentioned, but may successfully be used in computing the perturbations of the motions of other planets. For instance, in the computation of the perturbation of Saturn's motion by Jupiter, (and *vice versâ,*) the convergency of this series will be nearly by the powers of $\frac{1}{12}$, which is a swift rate of convergency. And, for the perturbation of the *Georgium sidus* by Saturn, (and *vice versâ,*) the series will converge nearly by the powers of $\frac{1}{9}$, which is also swiftly.

And it is further to be remarked, that in the last instance, and indeed whenever the radii of the orbits of the two planets differ from each other in the ratio of 2 to 1, M. De la Grange's series may be used with advantage, since the convergency of the first five terms of it will then be nearly by the powers of $\frac{1}{16}$; the numeral coefficients of those terms converging as

swiftly as the literal powers do in that case. And, when the ratio of the two radii is greater than that of 2 to 1, his series will converge more swiftly.

With great pleasure therefore I see, that, by one or other of these methods, some of the longest and most difficult calculations which formerly arose in the theory of astronomy, may now be exchanged for others which are short and easy.

It is with satisfaction also, that I perceive the facility of computing by the series I now present to you, is not at all lessened by the more general notation you have given to the denominator of the fraction from which it is derived, at the same time that a more accurate result is obtained than M. DE LA GRANGE proposed. For, in the computations of which I have been speaking, he neglected both the excentricities of the orbits of the planets, and their inclinations to the ecliptic, as inconsiderable: you, finding the effect of these omissions to be greater than he imagined, have taken them in. Your other ingenious labours on this subject will be best described by yourself, and cannot fail of being gratefully received by all learned astronomers.

With respect to the method by which the sums of the very slowly converging numerical series, which occur in the subsequent pages, are obtained, I need not say to you, that it is of extensive utility, and may be successfully applied in many cases.

I have only to request, that, if the paper here inclosed meets with your approbation, you will communicate it to the Royal Society. For, although I think I cannot be mistaken re-

specting the utility of the invention explained in it, yet such is my respect for that learned body, that I am unwilling to send them any paper of mine, on so difficult and important a subject, till it has been examined by an able judge of the subject.

I am,

Rev. Sir, *&c.*

JOHN HELLINS.

An improved Solution of a Problem in physical Astronomy, &c.

1. The perturbation of the motions of the planets in their orbits, by the action of one upon another, is a curious phænomenon, which, while it affords to the philosopher a clear proof of the general attraction of matter, produces a problem of no small difficulty to the astronomer; *viz.* to compute the quantity by which a planet, so acted upon, deviates from an ellipsis in its course round the sun: a problem which hath called forth the skill of several of the most learned philosophers and astronomers of the last and present age.

A preparatory step to the solution of this problem is, to find a convenient expression for the reciprocal of the cube, or rather of the n^{th} power, of the distance of any two planets. Such an expression was first given by M. EULER, in series proceeding by the cosines of the multiples, in arithmetic progression, of the angle of commutation; but the calculations of the first

two coefficients in it were very laborious, requiring the summation of series of the common form, which converged very slowly. Afterwards, other series were discovered by other authors, whereby the same coefficients might be computed with less labour; the best of which, that I have seen, appear to be those that were pointed out to me by Dr. MASKELYNE, invented by M. DE LA GRANGE, and published in the *Memoirs of the Royal Academy of Sciences at Berlin*, for the year 1781. Yet, the calculation of the two first coefficients, A and B, for the perturbations of Mars, Venus, and the Earth, by his method, is not shorter, if it be so short as by my method, to the investigation of which I now proceed.

PROBLEM.

2. *To determine the values of* A, B, C, D, &c. *in the equation*
$$\frac{\dot{z}}{(a-b\,.\,\cos.\,z)^n} = \dot{z}\,(A + B\,.\,\cos.\,z + C\,.\,\cos.\,2z + D\,.\,\cos.\,3z, \&c.)$$
z being the arch of a circle of which the radius is 1, *and* b *less than* a.

First, *to find the coefficient* A.

3. The fluent of the right-hand side of this equation is $Az + B\,.\,\sin.\,z + \frac{1}{2}C\,.\,\sin.\,2z + \frac{1}{3}D\,.\,\sin.\,3z + \frac{1}{4}E\,.\,\sin.\,4z$,* *&c.* which evidently vanishes when $z = 0$; and, when $z = 3\cdot14159$, *&c.* the arch of 180°, it becomes barely $= Az$, the sines of z, $2z$, $3z$, *&c.* being then each $= 0$. If, therefore, the fluent of the first side of the equation be taken, the increase of it, while z increases from 0 to $3\cdot14159$ *&c.* $= \pi$, will be $= \pi A$; and, consequently, A will be determined.

* See M. EULER's *Institutiones Calculi Integralis*, Vol. I. p. 150.

4. Now, to find the fluent of $\frac{\dot{z}}{(a-b\,.\,\cos.\,z)^n}$, we have $\frac{\dot{z}}{(a-b\,.\,\cos.\,z)^n}$ $=\frac{-\dot{x}}{\surd(1-xx)\,(a-bx)^n}$, x being put = the cosine of z; in which expressions, while z increases from 0 to 3·14159, x will decrease from 1 to $-$ 1. Therefore, to obtain a more convenient expression, put $vv=\frac{a-bx}{a+b}$; then, while x decreases from 1 to $-$ 1, vv will increase from $\frac{a-b}{a+b}$ to $\frac{a+b}{a+b}=1$; and we shall have the following equations:

$$a-bx=(a+b)\,vv,\ (a-bx)^n=(a+b)^n v^{2n},\ x=\frac{a-(a+b)vv}{b},-$$

$$\dot{x}=\frac{(a+b)2v\dot{v}}{b};\ 1+x=1+\frac{a-(a+b)vv}{b}=\frac{a+b-(a+b)vv}{b}=\frac{a+b}{b}(1-vv);$$

$$1-x=1-\frac{a-(a+b)vv}{b}=\frac{b-a+(a+b)vv}{b}=\frac{a+b}{b}\left(\frac{b-a}{a+b}+vv\right)=$$

$\frac{a+b}{b}\left(vv-\frac{a-b}{a+b}\right)=\frac{a+b}{b}(vv-cc)$, cc being put $=\frac{a-b}{a+b}$. And from these equations, the three following are easily obtained, viz.

$$\surd(1+x)\,\surd(1-x)=\surd\left(\frac{a+b}{b}(1-vv)\right)\times\surd\left(\frac{a+b}{b}(vv-cc)\right)$$

$=\frac{a+b}{b}\surd\Big((1-vv)\,(vv-cc)\Big)$; and,

$$\frac{-\dot{x}}{\surd(1-xx)}=\frac{(a+b)2v\dot{v}}{b}\times\frac{b}{(a+b)\surd\big((1-vv)\,(vv-cc)\big)}=\frac{2v\dot{v}}{\surd(1-vv)\surd(vv-cc)};$$

and, lastly, $\frac{-\dot{x}}{\surd(1-xx)\,(a-bx)^n}=\frac{2v\dot{v}}{\surd(1-vv)\,\surd(vv-cc)\,(a+b)^n v^{2n}}=$

$(a+b)^{-n}\times\frac{2\dot{v}v^{1-2n}}{\surd(1-vv)\surd(vv-cc)}$; the fluent of which may be found when the value of n is given.

5. Now, the values of n with which astronomers are most concerned, are $\frac{3}{2}$ and $\frac{5}{2}$. Let, therefore, $\frac{3}{2}$ be written for n, and the radical quantity $\surd(1-vv)$ be converted into series, and the last expression will be

$$=(a+b)^{-\frac{3}{2}}\times\frac{2\dot{v}v^{-2}}{\sqrt{(vv-cc)}}\left(1+\frac{vv}{2}+\frac{3v^4}{2.4}+\frac{3.5v^6}{2.4.6}+\frac{3.5.7v^8}{2.4.6.8},\ \&c.\right)$$

$$=(a+b)^{-\frac{3}{2}}\times\begin{cases}\dfrac{2\dot{v}v^{-2}}{\sqrt{(vv-cc)}}\\ +\dfrac{\dot{v}}{\sqrt{(vv-cc)}}\left(1+\dfrac{3vv}{4}+\dfrac{3.5v^4}{4.6}+\dfrac{3.5.7v^6}{4.6.8},\ \&c.\right)\end{cases}$$

And the fluents of these several terms, without their coefficients, are as follows:

$$\int\frac{\dot{v}v^{-2}}{\sqrt{(vv-cc)}}\text{ is }=\frac{\sqrt{(vv-cc)}}{ccv};$$

$$\int\frac{\dot{v}}{\sqrt{(vv-cc)}}=\text{H.L.}\ \frac{v+\sqrt{(vv-cc)}}{c}=\alpha;$$

$$\int\frac{\dot{v}v^2}{\sqrt{(vv-cc)}}=\frac{\sqrt{(vv-cc)}\,v+cc\alpha}{2}=\beta;$$

$$\int\frac{\dot{v}v^4}{\sqrt{(vv-cc)}}=\frac{\sqrt{(vv-cc)}\,v^3+3cc\beta}{4}=\gamma;$$

$$\int\frac{\dot{v}v^6}{\sqrt{(vv-cc)}}=\frac{\sqrt{(vv-cc)}\,v^5+5cc\gamma}{6}=\delta;$$

&c. &c.

These fluents, being multiplied by their proper coefficients, and collected together, and the whole multiplied by the common factor $(a+b)^{-\frac{3}{2}}$, the fluent sought will be

$$(a+b)^{-\frac{3}{2}}\times\begin{cases}\dfrac{2\sqrt{(vv-cc)}}{ccv}\\ +\alpha+\dfrac{3}{4}\beta+\dfrac{3.5}{4.6}\gamma+\dfrac{3.5.7}{4.6.8}\delta,\ \&c.\end{cases}$$

6. We must now inquire what value this series has when $z=0$; in which case, x being $=1$, vv is $=\frac{a-b}{a+b}=cc$. And it will appear that, with this value of vv, every term of the series vanishes, so that the fluent needs no correction. If, therefore, we compute the value of this series when $z=\pi$, *i. e.* when $x=-1$, and $vv=\frac{a+b}{a+b}=1$, we shall have the value of $A\pi$,

and consequently, A will be determined. But, with this value of v, the terms β, γ, δ, *&c.* lose all convergency by the geometrical progression v, v^3, v^5, *&c.* and the computation of the value of the series, by the common method, would be more laborious than the computation of the quadrantal arch of the circle, by the series $1 + \frac{1}{2.3} + \frac{3}{2.4.5} + \frac{3.5}{2.4.6.7}$, *&c.* Here then we are stopped. But, by contemplating this series, expressed in terms of α and c, as it stands below, a very different method of obtaining the value of it is suggested.

$$
\begin{array}{llll}
7.\ \alpha = & \alpha & & \\
\frac{3}{4}\beta = & \frac{3\sqrt{(1-cc)}}{4.2} & + \frac{3cc\alpha}{4.2} & \\
\frac{3.5}{4.6}\gamma = & \frac{3.5\sqrt{(1-cc)}}{4.6.4} & + \frac{3.5.3cc\sqrt{(1-cc)}}{4.6.4.2} & + \frac{3.5.3c^4\alpha}{4.6.4.2} \\
\frac{3.5.7}{4.6.8}\delta = & \frac{3.5.7\sqrt{(1-cc)}}{4.6.8.6} & + \frac{3.5.7.5cc\sqrt{(1-cc)}}{4.6.8.6.4} & + \frac{3.5.7.5.3c^4\sqrt{(1-cc)}}{4.6.8.6.4.2}, \&c. \\
\frac{3.5.7.9}{4.6.8.10}\varepsilon = & \frac{3.5.7.9\sqrt{(1-cc)}}{4.6.8.10.8} & + \frac{3.5.7.9.7cc\sqrt{(1-cc)}}{4.6.8.10.8.6} & + \frac{3.5.7.9.7.5c^4\sqrt{(1-cc)}}{4.6.8.10.8.6.4}, \&c. \\
\&c. & \&c. & \&c. & \&c.
\end{array}
$$

Here it appears, 1st. That the geometrical progression 1, cc, c^4, *&c.* has place in the first, second, third, *&c.* columns of quantities on the right-hand side of the equation, the terms of which, when b is nearly $= a$, decrease very swiftly.

2dly. That, in the diagonal line of quantities in which α enters, besides this decrease of the terms, by the literal powers before mentioned, the numeral coefficients are so simple that a considerable number of the terms may quickly be computed.

3dly. That, if this diagonal line of quantities be taken away, the first, second, third, *&c.* infinite columns of quantities which

remain will have the literal factors $\surd(1-cc)$, $cc\surd(1-cc)$, $c^4\surd(1-cc)$, *&c.* respectively, which are in the progression before mentioned.

4thly. That, if the sum of the infinite series of numeral coefficients below the line, in each of these columns, can be obtained, then the original series, which had lost all convergency by the literal powers v, v^3, v^5, *&c.* may be transformed into two others, in which the literal powers will be cc, c^4, *&c.*

8. But the sums of these infinite series are attainable, and are as follows:

$$\frac{3}{4.2} + \frac{3.5}{4.6.4} + \frac{3.5.7}{4.6.8.6} + \frac{3.5.7.9}{4.6.8.10.8}, \text{ \&c. is } = \tfrac{1}{2} + \text{H. L. } 2 = \lambda;$$

$$\frac{3.5.3}{4.6.4.2} + \frac{3.5.7.5}{4.6.8.6.4} + \frac{3.5.7.9.7}{4.6.8.10.8.6} + \frac{3.5.7.9.11.9}{4.6.8.10.12.10.8}, \text{ \&c. is } = \tfrac{11}{32} + \tfrac{7}{8}\text{H. L. } 2 = \mu;$$

$$\frac{3.5.7.5.3}{4.6.8.6.4.2} + \frac{3.5.7.9.7.5}{4.6.8.10.8.6.4} + \frac{3.5.7.9.11.9.7}{4.6.8.10.12.10.8.6}, \text{ \&c. is } = \frac{17 + 51\text{ H. L. } 2}{64} = \nu; \text{*}$$

&c. *&c.*

But these three sums are as many as are requisite, when the perturbation of the motion of either the Earth, Mars, or Venus, by the attraction of any one of the other, is to be computed.

9. The sum of the coefficients in the first, second, and third columns in Art. 7. being now obtained, take $\frac{8-2cc}{8-5cc}\alpha$ for the value of the series $\alpha\left(1 + \frac{3cc}{4.2} + \frac{3.5.3c^4}{4.6.4.2}, \text{ \&c.}\right)$, which will be exact enough for the purpose, and we shall have, by Art. 3, 5, 6, 7, and 8,

* See the Appendix.

$$\pi A = (a+b)^{-\frac{3}{2}} \times \begin{cases} \frac{2\sqrt{(1-cc)}}{cc} \\ + \frac{8-2cc}{8-5cc}\alpha \\ + \lambda\sqrt{(1-cc)} + \mu cc\sqrt{(1-cc)} + \nu c^4\sqrt{(1-cc)}; \end{cases}$$

and thence, by a more commodious arrangement of the terms, and dividing both sides by π,

$$A = \frac{1}{\pi(a+b)^{\frac{3}{2}}} \times \begin{cases} \frac{8-2cc}{8-5cc}\alpha \\ + \sqrt{(1-cc)}\left(\frac{2}{cc} + \lambda + \mu cc + \nu c^4\right) \end{cases}$$

10. The value of A, when $n = \frac{3}{2}$, being now found, let us next investigate the value of it when $n = \frac{5}{2}$; which, for the sake of distinction, in a use to be made of it in a subsequent article, I denote by A′.

By writing $\frac{5}{2}$ for n in the fluxionary expression obtained in Art. 4. we have $(a+b)^{-\frac{5}{2}} \times \frac{2\dot{v}v^{-4}}{\sqrt{(1-vv)}\sqrt{(vv-cc)}}$, which, by converting the radical quantity $\sqrt{(1-vv)}$ into series, becomes

$$(a+b)^{-\frac{5}{2}} \times \frac{2\dot{v}v^{-4}}{\sqrt{(vv-cc)}}\left(1 + \frac{vv}{2} + \frac{3v^4}{2.4} + \frac{3.5v^6}{2.4.6} + \frac{3.5.7v^8}{2.4.6.8}, \&c.\right)$$

$$= (a+b)^{-\frac{5}{2}} \times \begin{cases} \frac{2\dot{v}v^{-4}}{\sqrt{(vv-cc)}} + \frac{\dot{v}v^{-2}}{\sqrt{(vv-cc)}} \\ + \frac{\dot{v}}{\sqrt{(vv-cc)}}\left(\frac{3}{4} + \frac{3.5vv}{4.6} + \frac{3.5.7v^4}{4.6.8} + \frac{3.5.7.9v^6}{4.6.8.10}, \&c.\right) \end{cases}$$

Now, the fluents of these terms, without their coefficients, are as follows:

$$\int \frac{\dot{v}v^{-4}}{\sqrt{(vv-cc)}} \text{ is } = \frac{\sqrt{(vv-cc)}}{3ccv^3} + \frac{2\sqrt{(vv-cc)}}{3c^4v};$$

$$\int \frac{\dot{v}v^{-2}}{\sqrt{(vv-cc)}} = \frac{\sqrt{(vv-cc)}}{ccv};$$

$$\int \frac{\dot{v}}{\sqrt{(vv-cc)}} = \text{H. L.}\frac{v+\sqrt{(vv-cc)}}{c} = \alpha;$$

$$\int \frac{\dot{v}v^2}{\sqrt{(vv-cc)}} = \frac{\sqrt{(vv-cc)}v + cc\alpha}{2} = \mathfrak{C};$$ and the rest as they are exhibited in Art. 5.

These fluents being multiplied by their proper coefficients, and collected together, and their sum multiplied by the common factor $(a+b)^{-\frac{5}{2}}$, we shall have

$$(a+b)^{-\frac{5}{2}} \times \left\{ \begin{array}{l} \frac{2\sqrt{(vv-cc)}}{3ccv^3} + \frac{4\sqrt{(vv-cc)}}{3c^4 v} + \frac{\sqrt{(vv-cc)}}{ccv} \\ + \frac{3}{4}\alpha + \frac{3\cdot5}{4\cdot6}\beta + \frac{3\cdot5\cdot7}{4\cdot6\cdot8}\gamma + \frac{3\cdot5\cdot7\cdot9}{4\cdot6\cdot8\cdot10}\delta, \;\&c. \end{array} \right.$$

which fluent needs no correction, since, when $v=c$, the whole vanishes. The value of it therefore, when $v=1$, will be $= A'\pi$, which is what we want; and, in obtaining it, the only difficulty is, to compute the series in which α, β, γ, &c. enter, which may be overcome in the manner shewn above in Art. 7. For the value of this series, when $v=1$, will be as follows:

11.
$$\begin{array}{lllll}
\frac{3}{4}\alpha = & \frac{3}{4}\alpha & & & \\
\frac{3\cdot5}{4\cdot6}\beta = & \frac{3\cdot5\sqrt{(1-cc)}}{4\cdot6\cdot2} & + \frac{3\cdot5\,cc\alpha}{4\cdot6\cdot2} & & \\
\frac{3\cdot5\cdot7}{4\cdot6\cdot8}\gamma = & \frac{3\cdot5\cdot7\sqrt{(1-cc)}}{4\cdot6\cdot8\cdot4} & + \frac{3\cdot5\cdot7\cdot3\,cc\sqrt{(1-cc)}}{4\cdot6\cdot8\cdot4\cdot2} & + \frac{3\cdot5\cdot7\cdot3\,c^4\alpha}{4\cdot6\cdot8\cdot4\cdot2} & \\
\frac{3\cdot5\cdot7\cdot9}{4\cdot6\cdot8\cdot10}\delta = & \frac{3\cdot5\cdot7\cdot9\sqrt{(1-cc)}}{4\cdot6\cdot8\cdot10\cdot6} & + \frac{3\cdot5\cdot7\cdot9\cdot5\,cc\sqrt{(1-cc)}}{4\cdot6\cdot8\cdot10\cdot6\cdot4} & + \frac{3\cdot5\cdot7\cdot9\cdot5\cdot3\,c^4\sqrt{(1-cc)}}{4\cdot6\cdot8\cdot10\cdot6\cdot4\cdot2}, & \&c. \\
\frac{3\cdot5\cdot7\cdot9\cdot11}{4\cdot6\cdot8\cdot10\cdot12}\varepsilon = & \frac{3\cdot5\cdot7\cdot9\cdot11\sqrt{(1-cc)}}{4\cdot6\cdot8\cdot10\cdot12\cdot8} & + \frac{3\cdot5\cdot7\cdot9\cdot11\cdot7\,cc\sqrt{(1-cc)}}{4\cdot6\cdot8\cdot10\cdot12\cdot8\cdot6} & + \frac{3\cdot5\cdot7\cdot9\cdot11\cdot7\cdot5\,c^4\sqrt{(1-cc)}}{4\cdot6\cdot8\cdot10\cdot12\cdot8\cdot6\cdot4}, & \&c. \\
\&c. & \&c. & \&c. & \&c. &
\end{array}$$

Here, by attending to the same things which were observed in Art. 7, we may easily obtain the values of as many of the infinite columns of quantities on the right-hand side of the equation as are wanted; which, for the planets Mars, Venus, the Earth, and some of the rest, are but three.

12. The value of the series, of which α is the factor, *viz.* $\alpha\left(\frac{3}{4} + \frac{3\cdot5\,cc}{4\cdot6\cdot2} + \frac{3\cdot5\cdot7\cdot3\,c^4}{4\cdot6\cdot8\cdot4\cdot2}, \&c.\right)$, will be obtained sufficiently near for the purpose, by this expression, $\frac{96-23cc}{128-84cc}\alpha$, which is somewhat more exact than the three first terms of it; and the infinite series

$$\frac{3.5}{4.6.2} + \frac{3.5.7}{4.6.8.4} + \frac{3.5.7.9}{4.6.8.10.6} + \frac{3.5.7.9.11}{4.6.8.10.12.8}, \text{ \&c. is}$$
$$= \frac{9}{16} + \frac{3}{4}\text{H. L. } 2 = \lambda';$$

$$\frac{3.5.7.3}{4.6.8.4.2} + \frac{3.5.7.9.5}{4.6.8.10.6.4} + \frac{3.5.7.9.11.7}{4.6.8.10.12.8.6}, \text{ \&c. is}$$
$$= \frac{79}{192} + \frac{11}{16}\text{H. L. } 2 = \mu';$$

$$\frac{3.5.7.9.5.3}{4.6.8.10.6.4.2} + \frac{3.5.7.9.11.7.5}{4.6.8.10.12.8.6.4} + \frac{3.5.7.9.11.13.9.7}{4.6.8.10.12.14.10.8.6}, \text{ \&c. is}$$
$$= \frac{4067}{12288} + \frac{329}{512}\text{H.L. } 2 = \nu'.\text{*}$$

We therefore now have $\frac{96-23cc}{128-84cc}\alpha + \lambda'\sqrt{(1-cc)} + \mu' cc\sqrt{(1-cc)} + \nu' c^4\sqrt{(1-cc)}$ for a near value of the infinite series $\frac{3}{4}\alpha + \frac{3.5}{4.6}\beta + \frac{3.5.7}{4.6.8}\gamma$, &c.

13. Having thus obtained a sufficiently near value of the infinite series which entered into the fluent, in Art. 10, we have only to add to it the three radical terms there found, v being put $= 1$, and to multiply the whole by $(a+b)^{-\frac{5}{2}}$, and we shall have

$$\pi\Lambda' = (a+b)^{-\frac{5}{2}} \times \begin{cases} \frac{2\sqrt{(1-cc)}}{3cc} + \frac{4\sqrt{(1-cc)}}{3c^4} + \frac{\sqrt{(1-cc)}}{cc} \\ + \frac{96-23cc}{128-84cc}\alpha \\ + \lambda'\sqrt{(1-cc)} + \mu' cc\sqrt{(1-cc)} + \nu' c^4\sqrt{(1-cc)}; \end{cases}$$

which equation being more concisely expressed, and divided by π, gives

$$\Lambda' = \frac{1}{\pi\,(a+b)^{\frac{5}{2}}} \times \begin{cases} \frac{96-23cc}{128-84cc}\alpha \\ + \sqrt{(1-cc)}\left(\frac{4+5cc}{3c^4} + \lambda' + \mu' cc + \nu' c^4\right). \end{cases}$$

* See the Appendix.

Secondly, *to find the Coefficient* B.

14. Multiply the equation in Art. 2. by 2 cos. $z = 2x$, and we shall have $\frac{2x\dot{z}}{(a-bx)^n} = \dot{z}(A \times 2\cos.z + B \times 2\cos.z \times \cos.z + C \times 2\cos.z \times \cos.2z + D \times 2\cos.z \times \cos.3z$, &c.); which, because $2\cos.z \times \cos.mz$ is $= \cos.(m-1)z + \cos.(m+1)z$, will be $= \dot{z}(2A.\cos.z + B(1+\cos.2z) + C(\cos.z + \cos.3z) + D(\cos.2z + \cos.4z)$, &c.)* And, by taking the fluents, we have $\int \frac{2x\dot{z}}{(a-bx)^n} = 2A.\sin.z + Bz + \frac{1}{2}B.\sin.2z + C(\sin.z + \frac{1}{3}\sin.3z) + D(\frac{1}{2}\sin.2z + \frac{1}{4}\sin.4z)$, &c.; which equation, when $z = 3{\cdot}14159$, &c. $= \pi$, becomes $\int \frac{2x\dot{z}}{(a-bx)^n} =$ barely $Bz = B\pi$, the sines of z, $2z$, $3z$, &c. being then $= 0$.

15. Now it appears, by the notation in Art. 4, that $\frac{\dot{z}}{(a-bx)^n} = \frac{-\dot{x}}{\sqrt{(1-xx)}\,(a-bx)^n} = (a+b)^{-n} \times \frac{2\dot{v}v^{1-2n}}{\sqrt{(1-vv)}\sqrt{(vv-cc)}}$, and that $x = \frac{a-(a+b)vv}{b}$; we therefore have, by proper substitution,

$$\frac{2x\dot{z}}{(a-bx)^n} = \frac{-2x\dot{x}}{\sqrt{(1-xx)}\,(a-bx)^n} = \left.\begin{array}{l} \frac{2a}{b(a+b)^n} \times \frac{2\dot{v}v^{1-2n}}{\sqrt{(1-vv)}\sqrt{(vv-cc)}} \\ \frac{-2}{b(a+b)^{n-1}} \times \frac{2\dot{v}v^{3-2n}}{\sqrt{(1-vv)}\sqrt{(vv-cc)}} \end{array}\right\},$$

of which two fluxions the fluents may be found, when n has any particular value.

16. First, let n be $\frac{3}{2}$; then the last expression in the preceding article becomes $\frac{2a}{b(a+b)^{\frac{3}{2}}} \times \frac{2\dot{v}v^{-2}}{\sqrt{(1-vv)}\sqrt{(vv-cc)}} - \frac{2}{b(a+b)^{\frac{1}{2}}} \times \frac{2\dot{v}}{\sqrt{(1-vv)}\sqrt{vv-cc)}}$. Now, the fluent of the affirmative part of this expression is evidently $= \frac{2a}{b} \times$ the fluent of the fluxion in Art. 5,

* See SIMPSON's Miscellaneous Tracts, lemma I. p. 76.

that is, $=\frac{2a}{b}A\pi$; and the negative part, by converting $\surd(1-vv)$ into series, will become $\frac{-2}{b(a+b)^{\frac{1}{2}}}\times\frac{2\dot{v}}{\surd(vv-cc)}\left(1+\frac{vv}{2}+\frac{3v^4}{2.4}+\frac{3.5v^6}{2.4.6},\ \&c.\right)$; the fluent of which appears, by Art. 5, to be $\frac{-2}{b(a+b)^{\frac{1}{2}}}\left(\alpha+\beta+\frac{3\gamma}{4}+\frac{3.5\delta}{4.6}+\frac{3.5.7\varepsilon}{4.6.8},\ \&c.\right)$, which will vanish when $v=c$, and therefore needs no correction; and, when $v=1$, the series, without the factor, will be as follows:

$$2\alpha=2\alpha$$

$$\beta=\frac{\surd(1-cc)}{2}\qquad+\frac{cc\alpha}{2}$$

$$\frac{3}{4}\gamma=\frac{3\surd(1-cc)}{4.4}\qquad+\frac{3.3cc\surd(1-cc)}{4.4.2}\qquad+\frac{3.3c^4\alpha}{4.4.2}$$

$$\frac{3.5}{4.6}\delta=\frac{3.5\surd(1-cc)}{4.6.6}\qquad+\frac{3.5.5cc\surd(1-cc)}{4.6.6.4}\qquad+\frac{3.5.5.3c^4\surd(1-cc)}{4.6.6.4.2},\ \&c.$$

$$\frac{3.5.7}{4.6.8}\varepsilon=\frac{3.5.7\surd(1-cc)}{4.6.8.8}+\frac{3.5.7.7cc\surd(1-cc)}{4.6.8.8.6}+\frac{3.5.7.7.5c^4\surd(1-cc)}{4.6.8.8.6.4},\ \&c.$$

&c. &c. &c. &c.

Now, the sum of the infinite series

$$\frac{1}{2}+\frac{3}{4.4}+\frac{3.5}{4.6.6}+\frac{3.5.7}{4.6.8.8},\ \&c.\text{ being }=2\,\text{H.L.}\,2=\rho,$$

of $\frac{3.3}{4.4.2}+\frac{3.5.5}{4.6.6.4}+\frac{3.5.7.7}{4.6.8.8.6}+\frac{3.5.7.9.9}{4.6.8.10.10.8}$, &c. being $=\frac{3}{2}\text{H.L.}\,2=\sigma$,

of $\frac{3.5.5.3}{4.6.6.4.2}+\frac{3.5.7.7.5}{4.6.8.8.6.4}+\frac{3.5.7.9.9.7}{4.6.8.10.10.8.6}+\frac{3.5.7.9.11.11.9}{4.6.8.10.12.12.10.8}$, &c.

$=\frac{-1}{64}+\frac{41}{32}\text{H.L.}\,2=\tau$, &c. &c.

By proceeding as above, in Art. 9, a sufficiently near value of the whole series will be obtained in this expression,

$\frac{32-10cc}{16-9cc}\alpha+\surd(1-cc)\,(\rho+\sigma cc+\tau c^4)$; and this, multiplied by its proper factor, gives

$\frac{-2}{b(a+b)^{\frac{1}{2}}} \times \begin{cases} \frac{32-10cc}{16-9cc}\alpha \\ +\sqrt{(1-cc)}\,(\rho+\sigma cc+\tau c^4) \end{cases}$ for the other part of the fluent sought. And since, by Art. 14, this fluent is $= B\pi$, we have, by dividing both sides by π,

$$B = \frac{2a}{b}A - \frac{2}{\pi b(a+b)^{\frac{1}{2}}} \times \begin{cases} \frac{32-10cc}{16-9cc}\alpha \\ +\sqrt{(1-cc)}\,(\rho+\sigma cc+\tau c^4), \end{cases}$$ which

is its value when $n = \frac{3}{2}$.

17. We are next to find the value of this coefficient, when $n = \frac{5}{2}$; which, for the sake of distinction, I denote by B′.

With this value of n, the fluxionary expression in Art. 15, becomes $\frac{2a}{b(a+b)^{\frac{5}{2}}} \times \frac{2\dot{v}v^{-4}}{\sqrt{(1-vv)}\sqrt{(vv-cc)}} - \frac{2}{b(a+b)^{\frac{1}{2}}} \times \frac{2\dot{v}v^{-2}}{\sqrt{(1-vv)}\sqrt{(vv-cc)}}$; which being compared with the fluxions in Art. 5 and 10, it will appear that the fluent of the former part, when $v = 1$, is $= \frac{2a}{b}A'\pi$, and that the fluent of the latter part is $= \frac{-2}{b}A\pi$; which fluents, taken together, are, by Art. 14, $= B'\pi$. Therefore we have $B' = \frac{2a}{b}A' - \frac{2}{b}A = \frac{2}{b}(A'a - A)$.

Thirdly, *to find the Values of* C, D, E, *&c.*

18. The values of the coefficients A and B being now found, corresponding to the values of n $\frac{3}{2}$ and $\frac{5}{2}$, we might proceed in the same manner to find the value of C. For, if the equation in Art. 2, be multiplied by $2\cos. 2z$, and $\cos.(m-2)z + \cos.(m+2)z$ be written for $2\cos. 2z \times \cos. mz$, it will become $\frac{\dot{z}\times 2\cos.2z}{(a-b\times\cos.z)^n} = \dot{z}\,(2A\times\cos.2z + B(\cos.z+\cos.3z) + C(1+\cos.4z) + D(\cos.z+\cos.5z)$, *&c.*) And the sum

of the fluents on the right-hand side, when $z = \pi$, will become barely $Cz = C\pi$. Therefore, the fluent of the left-hand side of the equation, when $z = \pi$, or of $\frac{-\dot{x}(4xx-2)}{\sqrt{(1-xx)}\,(a-bx)^n}$, when $x=1$, or of $\frac{4aa-2bb-8a(a+b)vv+4(a+b)^2v^4}{bb(a+b)^n} \times \frac{2\dot{v}v^{1-2n}}{\sqrt{(1-vv)}\,\sqrt{(vv-cc)}}$, when $v = 1$, will be $= C\pi$. The fluent of this fluxion, it is evident, will consist of three parts, the first and second of which, n being $= \frac{3}{2}$, are obviously attainable from the values of A and B above found in Art. 9. and 16.; and the third in series similar to those which have been given in the former part of this paper.

It is evident also that, if n be $= \frac{5}{2}$, all three parts of this fluent are attainable from the values of the two coefficients already found, and C' would be $= -2A' + \frac{2}{b}(B'a - B)$.

19. And in this manner may the other coefficients, D, E, F, &c. be determined. And since the cosines of $3z$, $4z$, &c. are $= 4x^3 - 3x$, $8x^4 - 8x^2 + 1$, &c. respectively; and since $x = \frac{a-(a+b)vv}{b}$, it is evident that the numerator of the fraction into which the fluxion in the preceding article is to be multiplied, will be always of this form, *viz.* $p + qvv + rv^4 + sv^6$, &c.; from which it follows, that, if the values of A', A, $\underset{\prime}{A}$, &c. corresponding to n, $n-1$, $n-2$, &c. be computed, the values of C, D, E, F, and all the rest, may be found in terms of A', A, $\underset{\prime}{A}$, &c. with the coefficients a and b. But, since the easiest method, that has come to my hands, of computing the values of C, D, E, &c. after A and B are found, is explained in M. EULER's *Institutiones Calculi integralis*, Vol. I. p. 181,* I shall

* The coefficients 2, 3, and 4, after 2C *sin.* 3D *sin.* and 4E *sin.* in line 8 of the page above referred to, are wanting; and — is printed for + before 2C, in line 13. And there are press-errors in many other places. It is to be regretted, that so excellent a book was not more correctly printed.

not pursue this method any further; but, having examined his process, and corrected the errors of the press which occur in it, now give the equations expressing the values of C, D, E, F, &c. which were obtained by that method.

20. For the sake of brevity, let $\frac{a}{b} = d$; then will the general values of C, D, E, F, &c. be expressed by these equations:

$$C = \frac{2nA - 2dB}{n - 2}$$

$$D = \frac{(n+1)B - 4dC}{n - 3}$$

$$E = \frac{(n+2)C - 6dD}{n - 4}$$

$$F = \frac{(n+3)D - 8dE}{n - 5}$$

&c.

where the law of continuation is very obvious. And the particular values of these letters, when $n = \frac{1}{2}, \frac{3}{2}$, and $\frac{5}{2}$, will be as expressed in the following columns:

	$n = \frac{1}{2}$	$n = \frac{3}{2}$	$n = \frac{5}{2}$
$C =$	$\frac{4d}{3}B - \frac{4}{3}A$	$\frac{4d}{1}B - 6A$	$-4dB + 10A$
$D =$	$\frac{8d}{5}C - \frac{3}{5}B$	$\frac{8d}{3}C - \frac{5}{3}B$	$\frac{8d}{1}C - \frac{7}{1}B$
$E =$	$\frac{12d}{7}D - \frac{5}{7}C$	$\frac{12d}{5}D - \frac{7}{5}C$	$\frac{12d}{3}D - \frac{9}{3}C$
$F =$	$\frac{16d}{9}E - \frac{7}{9}D$	$\frac{16d}{7}E - \frac{9}{7}D$	$\frac{16d}{5}E - \frac{11}{5}D$
	&c.	&c.	&c.

21. The solution of the problem being now finished, it may perhaps be satisfactory to the reader to see how the sums of the very slowly converging numerical series, which arose in Art. 7, 11, and 16, were obtained; the investigations of which,

because they would have detained him too long from the immediate subject of this paper, if they had been inserted in it, are given in the following Appendix.

AN APPENDIX TO THE FOREGOING PAPER:

In which the Method of obtaining the Sums of the very slowly converging numerical Series which are used therein, and of many others of that Kind which arise in the Fluents of Binomial Surds, is explained and illustrated; and some Observations, tending to facilitate and abridge the Computations of the Coefficients A *and* B, *are added.*

1. As the sums of the very slowly converging numerical series, which arose in Art. 7, 11, and 16, of the preceding paper, are not exhibited in any book that has come to my hands, and as series of that kind frequently occur, I conceive that the following method of obtaining their sums will be acceptable to the lovers of mathematics in general, and particularly to those who have frequent occasion to use the sums of such series. And, having observed, while considering the literal expressions in the preceding paper for the values of A and B, that others, no less accurate, might be derived from them, by which the arithmetical operations would be facilitated and abridged, I thought these observations might likewise be acceptable to those who are engaged in the theory of astronomy, and have inserted them also in this paper; which, therefore, consists of two principal parts, the summation of the slowly converging series, and the observations now mentioned.

I. *The Summation of the slowly converging Series.*

2. But, before I begin the investigation, it will be proper to premise a few particulars, an attention to which will shorten and facilitate the operations now to be performed.

1st. That $\frac{1-\sqrt{(1-yy)}}{1+\sqrt{(1-yy)}}$ being $=\frac{1-\sqrt{(1-yy)}}{1+\sqrt{(1-yy)}}\times\frac{1+\sqrt{(1-yy)}}{1+\sqrt{(1-yy)}}$, is $=\left(\frac{y}{1+\sqrt{(1-yy)}}\right)^2$; from which it follows, that H. L. of $\frac{1-\sqrt{(1-yy)}}{1+\sqrt{(1-yy)}}$ is $=2$ H. L. $\frac{y}{1+\sqrt{(1-yy)}}$.

2dly. That the fluxion of H. L. $\frac{2}{1+\sqrt{(1-yy)}}$ is $=\frac{\dot{y}}{y\sqrt{(1-yy)}}-\frac{\dot{y}}{y}$. For it is $=$ the fluxion of $-$ H. L. $\left(1+\sqrt{(1-yy)}\right)$ $=\frac{y\dot{y}}{\sqrt{(-yy)}}\times\frac{1}{1+\sqrt{(1-yy)}}$; and, if both numerator and denominator of this expression be multiplied by $1-\sqrt{(1-yy)}$, it will become $\frac{y\dot{y}}{\sqrt{(1-yy)}}\times\frac{1-\sqrt{(1-yy)}}{yy}$, which is $=\frac{\dot{y}}{y\sqrt{(1-yy)}}$ $-\frac{\dot{y}}{y}$.

3dly. That the H. L. $\frac{2}{1+\sqrt{(1-yy)}}$ is therefore $=\int\frac{\dot{y}}{y\sqrt{(1-yy)}}$ $-\int\frac{\dot{y}}{y}=\frac{yy}{2.2}+\frac{3y^4}{2.4.4}+\frac{3.5y^6}{2.4.6.6}+\frac{3.5.7y^8}{2.4.6.8.8}$, &c.

4thly. That, Q being put $=\sqrt{(1-yy)}$, the fluxion of $\frac{Q}{y^n}$ will be $=\frac{\dot{y}}{Q}\left(\frac{-n}{y^{n+1}}+\frac{n-1}{y^{n-1}}\right)$. For it will be $\frac{\dot{Q}}{y^n}-\frac{n\dot{y}Q}{y^{n+1}}$ $=\frac{-y\dot{y}}{y^nQ}-\frac{n\dot{y}Q^2}{y^{n+1}Q}=\frac{-\dot{y}}{y^{n-1}Q}-\frac{n\dot{y}(1-yy)}{y^{n+1}Q}=\frac{-n\dot{y}}{y^{n+1}Q}+\frac{(n-1)\dot{y}}{y^{n-1}Q}$ $=\frac{\dot{y}}{Q}\left(\frac{-n}{y^{n+1}}+\frac{n-1}{y^{n-1}}\right)$.

5thly. That, when any quantities, as $\boxed{\dot{u}\left(\frac{f}{y^4}+\frac{g}{yy}+h\right)}$ are circumscribed by a parallelogram, it denotes that a substi-

tution for these quantities has been made in the same equation in which it occurs, and consequently that they are no longer to be considered as part of that equation. This I have found to be better than cancelling, as it answers the same end without obliteration.

We may now proceed to the summation of the series before-mentioned, in which the utility of what has been premised will quickly appear.

3. As it does not seem necessary to set down the operations of computing the sums of all the series which arose in the preceding paper, I will make choice of the summation of those which occur in Art. 11, they being the most difficult, as the properest examples to illustrate this method.

It is well known that the expression $\frac{2\dot{y}y^{-5}}{\sqrt{(1-yy)}}$ is $= 2\dot{y}y^{-5} + \frac{2\dot{y}y^{-3}}{2} + \frac{2.3\dot{y}y^{-1}}{2.4} + \frac{2.3.5\dot{y}y}{2.4.6} + \frac{2.3.5.7\dot{y}y^3}{2.4.6.8} + \frac{2.3.5.7.9\dot{y}y^5}{2.4.6.8.10}$, &c. from which equation we have $\frac{2\dot{y}y^{-5}}{(1-yy)} - 2\dot{y}y^{-5} - \dot{y}y^{-3} - \frac{3\dot{y}y^{-1}}{4} = \frac{3.5\dot{y}y}{4.6} + \frac{3.5.7\dot{y}y^3}{4.6.8} + \frac{3.5.7.9\dot{y}y^5}{4.6.8.10}$, &c. Now the fluents of the terms on the first side are

$$\left\{\begin{array}{l} \frac{-\sqrt{(1-yy)}}{2y^4} - \frac{3\sqrt{(1-yy)}}{4y^2} \boxed{+\frac{3}{4}\text{H.L.}\frac{y}{1+\sqrt{(1-yy)}}} \\ +\frac{1}{2y^4} + \frac{1}{2yy} \boxed{-\frac{3}{4}\text{H.L.}\,y} + \frac{3}{4}\text{H.L.}\frac{1}{1+\sqrt{(1-yy)}}; \end{array}\right.$$

on the second side, the fluents are $\frac{3.5yy}{4.6.2} + \frac{3.5.7y^4}{4.6.8.4} + \frac{3.5.7.9y^6}{4.6.8.10.6}$, &c. And, to find whether these two expressions are = each other, or have a constant difference, we may compute their numerical values, y being put = any small simple fraction, such as $\frac{1}{10}$, $\frac{1}{100}$, or $\frac{1}{1000}$, either of which values of y is a very convenient one for the purpose. But an easier method to dis-

cover the constant quantities which lie concealed in some of the terms on the first side, is to convert that side into series, by the binomial theorem; which will then be as follows:

$$\frac{-\sqrt{(1-yy)}}{2y^4} = -\tfrac{1}{2}y^{-4} + \tfrac{1}{4}y^{-2} + \tfrac{1}{16} + \tfrac{1}{32}y^2 + \tfrac{5}{256}y^4, \text{ \&c.}$$

$$\frac{-3\sqrt{(1-yy)}}{4yy} = \qquad -\tfrac{3}{4}y^{-2} + \tfrac{3}{8} + \tfrac{3}{32}y^2 + \tfrac{3}{64}y^4, \text{ \&c.}$$

$$+\frac{1}{2y^4} + \frac{1}{2yy} = +\tfrac{1}{2}y^{-4} + \tfrac{1}{2}y^{-2}$$

$$+\tfrac{3}{4}\text{H.L.}\frac{1}{1+\sqrt{(1-yy)}} = \qquad -\tfrac{3}{4}\text{H.L.}2 + \tfrac{3}{16}y^2 + \tfrac{9}{128}y^4, \text{\&c.}$$

$$\text{The sum is } = \quad * \quad * \quad + \tfrac{7}{16} - \tfrac{3}{4}\text{H.L.}2, + \tfrac{5}{16}y^2 + \tfrac{35}{256}y^4, \text{\&c.}$$

which evidently differs from the series on the second side by the constant quantity $\frac{7}{16} - \frac{3}{4}$ H. L. 2. We therefore have, by subtracting this constant quantity from the first side,

$$\left.\begin{array}{l} \frac{-\sqrt{(1-yy)}}{2y^4} - \frac{3\sqrt{(1-yy)}}{4yy} + \frac{3}{4}\text{H.L.}\frac{2}{1+\sqrt{(1-yy)}} \\ + \frac{1}{2y^4} + \frac{1}{2yy} - \frac{7}{16} \end{array}\right\} = \frac{3\cdot5yy}{4.6.2} + \frac{3\cdot5\cdot7y^4}{4.6.8.4} + \frac{3\cdot5\cdot7\cdot9y^6}{4.6.8.10.4}, \text{\&c.}$$

which, when y becomes $= 1$, becomes

$$\left.\begin{array}{l} * \qquad * \ + \tfrac{3}{4}\text{H. L. } 2 \\ \left| + \tfrac{1}{2} + \tfrac{1}{2} - \tfrac{7}{16} \right| + \tfrac{9}{16} \end{array}\right\} = \frac{3\cdot5}{4.6.2} + \frac{3\cdot5\cdot7}{4.6.8.4} + \frac{3\cdot5\cdot7\cdot9}{4.6.8.10.6}, \text{\&c.}$$

which is the series denoted by λ' in Art. 12. of the preceding paper.

4. If the equation of fluents in the preceding Article be divided by y, and if $\frac{3.5y}{4.6.2} = \frac{5}{16}y$ be then taken from both sides of it, and u be written for H. L. $\frac{2}{1+\sqrt{(1-yy)}}$, we shall have

$$\left.\begin{array}{l} \frac{-\sqrt{(1-yy)}}{2y^5} - \frac{3\sqrt{(1-yy)}}{4y^3} + \frac{3u}{4y} \\ + \frac{1}{2y^5} + \frac{1}{2y^3} - \frac{7}{16y} - \frac{5y}{16} \end{array}\right\} = \frac{3\cdot5\cdot7y^3}{4.6.8.4} + \frac{3\cdot5\cdot7\cdot9y^5}{4.6.8.10.6} + \frac{3\cdot5\cdot7\cdot9\cdot11y^7}{4.6.8.10.12.8}, \text{\&c.}$$

And, if this equation be put into fluxions, and Q be written for $\sqrt{(1-yy)}$, for the sake of brevity, there will be

$$\left.\begin{matrix}\frac{5}{2y^6}+\frac{9}{4y^4}+\frac{3}{4yy}\\ -\frac{4}{2y^4}-\frac{6}{4yy}\end{matrix}\right\}\frac{\dot{y}}{Q}\boxed{+\frac{3\dot{u}}{4y}}-\frac{3u\dot{y}}{4yy}$$

$$+\left(-\frac{5}{2y^6}+\frac{3}{2y^4}\begin{matrix}+\frac{7}{16yy}\\ -\frac{3}{4yy}\end{matrix}-\frac{5}{16}\right)\dot{y}=\frac{3.5.7.3\dot{y}yy}{4.6.8.4}+\frac{3.5.7.9.5\dot{y}y^4}{4.6.8.10.6}$$

$$+\frac{3.5.7.9.11.7\dot{y}y^6}{4.6.8.10.12.8},\ \&c.$$

And this equation, more concisely expressed and divided by y, gives

$$\left(\frac{5}{2y^7}+\frac{1}{4y^5}-\frac{3}{4y^3}\right)\frac{\dot{y}}{Q}-\frac{3u\dot{y}}{4y^3}$$

$$+\left(\frac{-5}{2y^7}-\frac{3}{2y^5}-\frac{5}{16y^3}-\frac{5}{16y}\right)\dot{y}=\frac{3.5.7.3\dot{y}y}{4.6.8.4}+\frac{3.5.7.9.5\dot{y}y^3}{4.6.8.10.6}+\frac{3.5.7.9.11.7\dot{y}y^5}{4.6.8.10.12.8},\ \&c.$$

Now the fluent of the series on the second side of this equation is found, by the methods which have been long known, to be $\frac{3.5.7.3yy}{4.6.8\ 4.2}+\frac{3.5.7.9.5y^4}{4.6.8.10.0.4}+\frac{3.5.7.9.11.7y^6}{4.6.8.10.12.8.6}$, &c. and the fluent of the terms on the first side will be very easily obtained, by the following assumption, and attention to what was shewn in Art. 2. of this paper.

For the fluent of the terms on the first side of this equation, assume

$$\left(\frac{a}{y^6}+\frac{b}{y^4}+\frac{c}{y^2}\right)Q+u\left(\frac{f}{yy}+g\right)$$

$+\frac{p}{y^6}+\frac{q}{y^4}+\frac{r}{yy}+s\,\text{H. L.}\,y$; then will the fluxion of this expression be

$$\left.\begin{matrix}\frac{-6a}{y^7}-\frac{4b}{y^5}-\frac{2c}{y^3}\\ +\frac{5a}{y^5}+\frac{3b}{y^3}+\frac{c}{y}\\ +\frac{f}{y^3}+\frac{g}{y}\end{matrix}\right\}\frac{\dot{y}}{Q}\ \boxed{+\dot{u}\left(\frac{f}{yy}+g\right)}\ -\frac{2fu\dot{y}}{y^3}$$

$$+\left.\begin{array}{l}\left(\frac{-6p}{y^7}-\frac{4q}{y^5}-\frac{2r}{y^3}+\frac{s}{y}\right. \\ \qquad\qquad -\frac{f}{y^3}-\frac{g}{y}\end{array}\right\}\dot{y},$$ which being put = the first side of

the foregoing equation, there will arise as many simple equations for determining the coefficients a, b, c, &c. as there are letters of that kind in the assumed fluent, from which their values will easily be found. For there will be

$$
\begin{array}{ll}
6a = \frac{-5}{2}, \text{ from which} & a = \frac{-5}{12}, \\
4b = 5a - \frac{1}{4}, & b = \frac{-7}{12}, \\
2c = 3b + f + \frac{3}{4}, & c = \frac{-5}{16}, \\
2f = \frac{3}{4}, & f = \frac{3}{8}, \\
g = -c, & g = \frac{5}{16}, \\
6p = \frac{5}{2}, & p = \frac{5}{12}, \\
4q = \frac{3}{2}, & q = \frac{3}{8}, \\
2r = \frac{5}{16} - f, & r = \frac{-1}{32}, \\
s = g - \frac{5}{16}, & s = 0.
\end{array}
$$

The variable part, therefore, of the fluent of the first side of the above equation is

$$Q\left(\frac{-5}{12y^6}-\frac{7}{12y^4}-\frac{5}{16yy}\right)+u\left(\frac{3}{8yy}+\frac{5}{16}\right)$$
$$+\frac{5}{12y^6}+\frac{3}{8y^4}-\frac{1}{32yy}.$$

Now, to discover the constant quantities which lie concealed in this expression, we must proceed as above in Art. 3.

$$\frac{-5\sqrt{(1-yy)}}{12y^6} \text{ is} = -\frac{5}{12}y^{-6} + \frac{5}{12.2}y^{-4} + \frac{5}{12.8}y^{-2} + \frac{5}{12.16} + \frac{5.5}{12.8.16}y^2, \text{ \&c.}$$

$$\frac{-7\sqrt{(1-yy)}}{12y^4} = -\frac{7}{12}y^{-4} + \frac{7}{12.2}y^{-2} + \frac{7}{12.8} + \frac{7}{12.16}y^2, \text{ \&c.}$$

$$\frac{-5\sqrt{(1-yy)}}{16yy} = -\frac{5}{16}y^{-2} + \frac{5}{16.2} + \frac{5}{16.8}y^2, \text{ \&c.}$$

$$u\left(\frac{3}{8yy} + \frac{5}{16}\right)^* = +\frac{3}{4.8} + \frac{29}{8.32}y^2, \text{ \&c.}$$

$$\left.\begin{matrix}\text{To which add the}\\ \text{other terms}\ -\end{matrix}\right\} + \frac{5}{12}y^{-6} + \frac{3}{8}y^{-4} - \frac{1}{32}y^{-2}$$

$$\text{The sum is} \quad * \quad * \quad * + \frac{67}{192} + \frac{105}{512}y^2, \text{ \&c.}$$

which exceeds the series above found, by the constant quantity $\frac{67}{192}$. We therefore now have

$$Q\left(\frac{-5}{12y^6} - \frac{7}{12y^4} - \frac{5}{16yy}\right) + u\left(\frac{3}{8yy} + \frac{5}{16}\right)$$
$$+ \frac{5}{12y^6} + \frac{3}{8y^4} - \frac{1}{32yy} - \frac{67}{192} = \frac{3.5.7.3\,yy}{4.6.8.4.2} + \frac{3.5.7.9.5\,y^4}{4.6.8.10.6.4} +$$
$\frac{3.5.7.9.11.7y^6}{4.6.8.10.12.8.6}$, &c.; and when y becomes $= 1$, Q being then $= 0$, and $u =$ H. L. 2, this equation becomes

$$\left.\begin{matrix}\text{H. L. } 2\left(\frac{3}{8} + \frac{5}{16}\right)\\ + \frac{5}{12} + \frac{3}{8} - \frac{1}{32} - \frac{67}{192}\end{matrix}\right\} = \left.\begin{matrix}\frac{11}{16}\text{H. L. } 2\\ + \frac{79}{192}\end{matrix}\right\} = \frac{3.5.7.3}{4.6.8.4.2} + \frac{3.5.7.9.5}{4.6.8.10.6.4} + \frac{3.5.7.9.11.7}{4.6.8.10.12.8.6}, \text{ \&c.}$$

which is the value of μ' in Art. 12. of the preceding paper.

5. If the last literal equation be divided by y, and $\frac{3.5.7.3y}{4.6.8.4.2} = \frac{105y}{512}$ be then taken from both sides, we shall have

$$\left.\begin{matrix}Q\left(\frac{-5}{12y^7} - \frac{7}{12y^5} - \frac{5}{16y^3}\right) + u\left(\frac{3}{8y^3} + \frac{5}{16y}\right)\\ + \frac{5}{12y^7} + \frac{3}{8y^5} - \frac{1}{32y^3} - \frac{67}{192y} - \frac{105y}{512}\end{matrix}\right\} =$$

* $u\left(\frac{3}{8yy} + \frac{5}{16}\right)$ is $= \left(\frac{1}{4}yy + \frac{3}{32}y^4, \text{ \&c.}\right)\left(\frac{3}{8}y^{-2} + \frac{5}{16}\right)$. See Art. 2.

$$\frac{3.5.7.9.5y^3}{4.6.8.10.6.4}+\frac{3.5.7.9.11.7y^5}{4.6.8.10.12.8.6.}+\frac{3.5.7.9.11.13.9y^7}{4.6.8.10.12.14.10.8}, \text{ \&c.}$$

which equation, in fluxions, gives

$$\left.\begin{array}{l}\frac{5.7}{12y^8}+\frac{7.5}{12y^6}+\frac{5.3}{16y^4}\\ \quad -\frac{5.6}{12y^6}-\frac{7.4}{12y^4}-\frac{5.2}{16yy}\\ \qquad\qquad +\frac{3}{8y^4}+\frac{5}{16yy}\end{array}\right\}\frac{\dot{y}}{Q}\quad \begin{array}{l}\boxed{+\dot{u}\left(\frac{3}{8y^3}+\frac{5}{16y}\right)}\\ -u\dot{y}\left(\frac{9}{8y^4}+\frac{5}{16yy}\right)\end{array}$$

$$+\left(\begin{array}{l}-\frac{5.7}{12y^8}-\frac{3.5}{8y^6}+\frac{3}{32y^4}+\frac{67}{192yy}-\frac{105}{512}\\ \qquad\qquad -\frac{3}{8y^4}-\frac{5}{16yy}\end{array}\right\}\dot{y}=$$

$$\frac{3.5.7.9.5.3\dot{y}yy}{4.6.8.10.6.4}+\frac{3.5.7.9.11.7.5\dot{y}y^4}{4.6.8.10.12.8.6}+\frac{3.5.7.9.11.13.9.7\dot{y}y^6}{4.6.8.10.12.14.10.8}, \text{ \&c.}$$

And this equation, more concisely expressed, and divided by y, gives

$$\frac{\dot{y}}{Q}\left(\frac{35}{12y^9}+\frac{5}{12y^7}-\frac{49}{48y^5}-\frac{5}{16y^3}\right)-u\dot{y}\left(\frac{9}{8y^5}+\frac{5}{16y^3}\right)$$

$$+\dot{y}\left(-\frac{35}{12y^9}-\frac{15}{8y^7}-\frac{9}{32y^5}+\frac{7}{192y^3}-\frac{105}{512y}\right)=$$

$$\frac{3.5.7.9.5.3\dot{y}y}{4.6.8.10.6.4}+\frac{3.5.7.9.11.7.5\dot{y}y^3}{4.6.8.10.12.8.6}+\frac{3.5.7.9.11.13.9.7\dot{y}y^5}{4.6.8.10.12.14.10.8}, \text{ \&c.}$$

Now the fluent of the fluxionary series on the second side of the equation being obviously the series $\frac{3.5.7.9.5.3yy}{4.6.8.10.6.4.2}+\frac{3.5.7.9.11.7.5y^4}{4.6.8.10.12.8.6.4}+\frac{3.5.7.9.11.13.9.7y^6}{4.6.8.10.12.14.10.8.6}$, &c. we are next to take the fluent of the expression on the first side, and to correct it, that it may be = this series; which may be done as follows:

For the fluent sought, assume

$$Q\left(\frac{a}{y^8}+\frac{b}{y^6}+\frac{c}{y^4}+\frac{d}{yy}\right)+u\left(\frac{f}{y^4}+\frac{g}{yy}+h\right)$$

$+\frac{p}{y^8}+\frac{q}{y^6}+\frac{r}{y^4}+\frac{s}{yy}+t$ H. L. y, and take the fluxion of this expression, which will be

$$\left.\begin{array}{l}\dfrac{-8a}{y^9}-\dfrac{6b}{y^7}-\dfrac{4c}{y^5}-\dfrac{2d}{y^3}\\ \qquad+\dfrac{7a}{y^7}+\dfrac{5b}{y^5}+\dfrac{3c}{y^3}+\dfrac{d}{y}\\ \qquad\qquad+\dfrac{f}{y^5}+\dfrac{g}{y^3}+\dfrac{h}{y}\end{array}\right\}\frac{\dot{y}}{Q}\ \boxed{+\dot{u}\left(\frac{f}{y^4}+\frac{g}{yy}+h\right)}\ -u\dot{y}\left(\frac{4f}{y^5}+\frac{2g}{y^3}\right)$$

$$+\left(\begin{array}{l}\dfrac{-8p}{y^9}-\dfrac{6q}{y^7}-\dfrac{4r}{y^5}-\dfrac{2s}{y^3}+\dfrac{t}{y}\\ \qquad-\dfrac{f}{y^5}-\dfrac{g}{y^3}-\dfrac{h}{y}\end{array}\right\}\dot{y};$$ and these fluxionary terms being put $=$ to those on the first side of the preceding equation, there will arise

$8a=\frac{-35}{12}$,	from which	$a=\frac{-35}{8.12}$,
$6b=7a-\frac{5}{12}$,		$b=\frac{-95}{2.8.12}$,
$4c=5b+f+\frac{49}{48}$,		$c=\frac{-75}{4.8.8}$,
$2d=3c+g+\frac{5}{16}$,		$d=\frac{-105}{8.8.8}$,
$4f=\frac{9}{8}$,		$f=\frac{9}{32}$,
$2g=\frac{5}{16}$,		$g=\frac{5}{32}$,
$h=-d$,		$h=\frac{105}{8.8.8}$,
$8p=\frac{35}{12}$,		$p=\frac{35}{8.12}$,
$6q=\frac{15}{8}$,		$q=\frac{5}{16}$,
$4r=\frac{9}{32}-f$,		$r=0$,
$2s=\frac{-7}{192}-g$,		$s=\frac{-37}{4.8.12}$,
$t=\frac{-105}{512}+h$,		$t=0$.

Our assumed fluent, therefore, is

$$Q\left(\frac{-35}{8.12y^8}-\frac{95}{12.16y^6}-\frac{75}{4.8.8y^4}-\frac{105}{8.8.8yy}\right)+u\left(\frac{9}{32y^4}+\frac{5}{32yy}+\frac{105}{8.8.8}\right)$$

$$+\frac{35}{8.12y^8}+\frac{5}{16y^6} \quad * \quad -\frac{37}{4.8.12yy} \quad *,$$ which may be corrected in the manner shewn in the two preceding Articles, or more expeditiously, as follows.

It is pretty evident, from the correction of the fluent in the preceding Article, that the constant quantities which lie concealed in this fluent, will appear in those terms only, (when the radical quantity $\sqrt{(1-yy)}$, and the logarithm u, is expressed in series,) in which the index of y is o. Thus, the constant quantities will appear as below.

The 5th term of $\frac{-35\sqrt{(1-yy)}}{8.12y^8}$ is $\frac{35}{8.12}\times\frac{5y^8}{8.16y^8}=\frac{175}{4.12.16.16}$;

The 4th term of $\frac{-95\sqrt{(1-yy)}}{12.16y^6}$ is $\frac{95}{12.16}\times\frac{y^6}{16y^6}=\frac{95}{12.16.16}$;

The 3d term of $\frac{-75\sqrt{(1-yy)}}{4.8.8y^4}$ is $\frac{75}{4.8.8}\times\frac{y^4}{8y^4}=\frac{75}{8.16.16}$;

The 2d term of $\frac{-105\sqrt{(1-yy)}}{8.8.8yy}$ is $\frac{105}{8.8.8}\times\frac{yy}{2yy}=\frac{105}{4.16.16}$;

and the terms in which the index of y is o, in the logarithmic part, *viz.* $\left(\frac{1}{4}yy+\frac{3}{2.16}y^4, \text{\&c.}\right)\left(\frac{9}{2.16}y^{-4}+\frac{5}{2.16}y^{-2}+\frac{105}{8.8.8}\right.$,

are these two, - - $\frac{27y^4}{4.16.16y^4}=\frac{27}{4.16.16}$,

and $\frac{5yy}{8.16yy}=\frac{5}{8.16}$.

The sum of these six fractions is $\frac{1023}{4096}$. The equation of fluents therefore is

$$Q\left(\frac{-35}{8.12y^8}-\frac{95}{12.16y^6}-\frac{75}{16.16y^4}-\frac{105}{8.8.8yy}\right)+u\left(\frac{9}{32y^4}+\frac{5}{32yy}+\frac{105}{8.8.8}\right)$$

$$+\frac{35}{8.12y^8}+\frac{5}{16y^6} \quad * \quad -\frac{37}{4.8.12yy}-\frac{1023}{4096}=$$ the series

$$\frac{3.5.7.9.5.3\,yy}{4.6.8.10.6.4.2} + \frac{3.5.7.9.11.7.5\,y^4}{4.6.8.10.12.8.6.4} + \frac{3.5.7.9.11.13.9.7\,y^6}{4.6.8.10.12.14.10.8.6}, \text{ \&c.}$$

which, when $y = 1$, becomes

$$\left.\begin{array}{l} \text{H. L. } 2\left(\frac{9}{32} + \frac{5}{32} + \frac{105}{8.8.8}\right) \\ + \frac{35}{8.12} + \frac{5}{16} - \frac{37}{4.8.12} - \frac{1023}{4096} \end{array}\right\} = \frac{4067}{12288} + \frac{329}{512}\text{ H. L. } 2$$

$$= \frac{3.5.7.9.5.3}{4.6.8.10.6.4.2} + \frac{3.5.7.9.11.7.5}{4.6.8.10.12.8.6.4} + \frac{3.5.7.9.11.13.9.7}{4.6.8.10.12.14.10.8.6}, \text{ \&c. which is}$$

the value of ν' in Art. 12. of the foregoing paper.

These three examples, I conceive, are sufficient to illustrate this method of summing the slowly converging numerical series which arose in the solution of the problem in the preceding paper. The three series of which the sums are now investigated are, as was before observed, the most difficult to sum of all that arose in that solution; so that, whoever understands what is done here, may, with great ease, compute the sums of the rest of the series which are found there, and of many others of this kind, which arise in the solution of problems.

II. *Observations, tending to facilitate and abridge the numerical Computations of* A *and* B *in the preceding Paper.*

6. The radical factor $\sqrt{(1 - cc)}$, in the literal expressions of the values of A and B, may be taken away, by multiplying the other factors by its equivalent $1 - \frac{cc}{2} - \frac{c^4}{8} - \frac{c^6}{16}$, *&c.* in consequence of which, other expressions will be obtained, better adapted to the purpose of numerical calculation. This will appear by the following operations.

The product of $\sqrt{(1 - cc)} \times$ the other factor in the expression of the value of A, in Art. 9. of the preceding paper, will be

$$\frac{\dfrac{2}{cc}+\lambda+\mu cc+\nu c^4}{1-\dfrac{cc}{2}-\dfrac{c^4}{8}-\dfrac{c^6}{16},\ \&c.}$$

$$\left.\begin{array}{l} \frac{2}{cc}+\lambda+\ \mu cc+\quad \nu c^4 \\ \quad -1-\frac{1}{2}\lambda cc-\frac{1}{2}\mu c^4 \\ \qquad\quad -\frac{1}{4}\ cc\ -\frac{1}{8}\lambda c^4 \\ \qquad\qquad\qquad -\frac{1}{8}\ c^4 \end{array}\right\}=\frac{2}{cc}+e+fcc+gc^4,\ \&c.$$

where e, f, and g, are $=\lambda-1$, $\mu-\frac{1}{2}\lambda-\frac{1}{4}$, and $\nu-\frac{1}{2}\mu-\frac{1}{8}\lambda-\frac{1}{8}$, respectively; in numbers $=$ 0·1931472, 0·1036802, and 0·0687064, respectively. And this expression, which is evidently more simple than the former, is somewhat nearer than that to the value of the whole series, as will appear to any one who shall compute the value of the next coefficient.

7. In like manner, the product of the two factors in the value of A′, in Art. 13. will be

$$\frac{\dfrac{4}{3c^4}+\dfrac{5}{3cc}+\lambda'+\mu'cc+\nu'c^4}{1-\dfrac{cc}{2}-\dfrac{c^4}{8}-\dfrac{c^6}{16}-\dfrac{5c^8}{8.16},\ \&c.}$$

$$\left.\begin{array}{l} \frac{4}{3c^4}+\frac{5}{3cc}+\lambda'+\ \mu'cc+\quad \nu'c^4 \\ \quad -\frac{2}{3cc}-\frac{5}{6}-\frac{1}{2}\lambda'cc-\frac{1}{2}\mu'c^4 \\ \qquad\qquad -\frac{1}{6}-\frac{5}{24}\ cc\ -\frac{1}{8}\lambda'c^4 \\ \qquad\qquad\qquad -\frac{1}{12}\ cc\ -\frac{5}{48}\ c^4 \\ \qquad\qquad\qquad\qquad\quad -\frac{5}{96}\ c^4 \end{array}\right\}=\frac{4}{3c^4}+\frac{1}{cc}+h+icc+kc^4,\ \&c.$$

which expression also is more simple than that from which it is derived, while the accuracy of it is not less, as is pretty evident on inspection. And, that the numerical values of h, i, and k, are very easily attainable from the values of λ', μ', and ν',

given above in Art. 3, 4, and 5, of this paper, is very obvious. In a subsequent Article, these values will be inserted.

8. And the product of the two factors in the value of B, in Art. 16, may also be exchanged for a more convenient expression, by a like process.

$$\frac{\begin{array}{lll} \rho + & \sigma cc + & \tau c^4 \\ 1 - & \frac{cc}{2} - & \frac{c^4}{8}, \&c. \end{array}}{\left.\begin{array}{rrr} \rho + & \sigma cc + & \tau c^4 \\ & -\frac{1}{2}\rho cc - & \frac{1}{2}\sigma c^4 \\ & & -\frac{1}{8}\rho c^4 \end{array}\right\} = \rho + lcc + mc^4, \&c.}$$

which expression also is more accurate than that from which it is derived, as well as more simple. The numerical values of l and m, which are evidently given from those of ρ, σ, and τ, will be inserted a little further on, when we come to an example of calculating the values of A and B in numbers.

9. The numerical calculation of the other member also, in which α enters, may be facilitated and abridged, by the following considerations.

If c be put for the sine of an angle, radius being 1, then will $1+\sqrt{(1-cc)}$ be the versed-sine of the supplement of that angle, and $\frac{c}{1+\sqrt{(1-cc)}}$ will be $=$ the tangent of half that angle; from which it follows, that the reciprocal of this quantity, *viz.* $\frac{1+\sqrt{(1-cc)}}{c}$, is $=$ the co-tangent of half the angle of which the sine is c. The common logarithm of $\frac{1+\sqrt{(1-cc)}}{c}$ may therefore be taken out from TAYLOR's excellent Tables,* and quickly converted into an hyperbolic logarithm, by Table XXXVII. of DODSON's Calculator.

* These valuable Tables are computed to every second of the quadrant.

10. An expression of this kind, $\frac{p \pm rcc}{q \pm scc}$, when c is the only variable quantity, consisting of several figures, and r and s are likewise long numbers, will be much better adapted to the use of logarithms, when put in this form, $\frac{r}{s} \times \frac{p \div r \pm cc}{q \div s \pm cc}$; because the multiplications of r and s into cc, or additions of their logarithms and taking out two numbers, are by this means exchanged for the addition of the constant logarithm of $\frac{r}{s}$: the quotients $\frac{p}{r}$ and $\frac{q}{s}$, once found, being constant numbers. Thus, the numerical value of even $\frac{8-2cc}{8-5cc}$, where r and s are single figures, is more easily obtained by $\frac{4}{10} \times \frac{4-cc}{1\cdot6-cc}$, than by the former expression.

11. But it will appear upon trial, that the arithmetical value of any three terms $p' + q'cc + r'c^4$, in which p', q', and r', are constant quantities, and cc consists of five or six places of figures, may, in general, be more easily obtained by logarithms than the arithmetical value of $\frac{r}{s} \times \frac{p \div r \pm cc}{q \div s \pm cc}$. And, since the difference of the values of these two expressions is inconsiderable in the present case, I shall make no further use of the fractional expression; but observe, that the logarithm of $q'cc$, in the other expression, being found, the logarithm of $r'c^4$ will be had, by adding to it the logarithm of $\frac{r'}{q'}cc$; for $q'cc \times \frac{r'}{q'}cc = r'c^4$. And, since the logarithms of the numbers which stand in the places of q' and $\frac{r'}{q'}$ may be taken out and reserved for use, and the logarithms of cc and α, once found, will serve for all the terms in which these quantities occur, it will appear by an example, that neither many logarithms, nor many numbers correspond-

ing to logarithms, need be taken out of other tables, in computing the value of A or B.

12. It will now be proper, since the literal expressions of the values of A and B have been exchanged for others which are more convenient, to bring the new equations together in one view, and, after that, to give an example of the numerical calculations by them.

It appears, by Art. 9, 10, 13, 16, and 17 of the preceding paper, and 6, 7, and 8 of this, that

$$1.\quad A = \frac{1}{\pi(a+b)^{\frac{3}{2}}} \times \begin{cases} \frac{2}{cc} + e + fcc + gc^4 \\ \qquad + \alpha + \frac{3}{8}\alpha cc + \frac{3.5}{8.8}\alpha c^4. \end{cases}$$

$$2.\quad A' = \frac{1}{\pi(a+b)^{\frac{5}{2}}} \times \begin{cases} \frac{4}{3c^4} + \frac{1}{cc} + h + icc + kc^4 \\ \qquad + \frac{3}{4}\alpha + \frac{3.5}{4.12}\alpha cc + \frac{3.5.21}{4.12.32}\alpha c^4. \end{cases}$$

$$3.\quad B = \frac{2a}{b}A - \frac{2}{\pi b(a+b)^{\frac{1}{2}}} \times \begin{cases} \rho + lcc + mc^4 \\ + 2\alpha + \frac{1}{2}\alpha cc + \frac{9}{2.16}\alpha c^4. \end{cases}$$

$$4.\quad B' = \frac{2}{b}(A'a - A).$$

In which equations, the values of the coefficients are as follows:

$e = 0{\cdot}1931472$, $\quad h = 0{\cdot}0823604$, $\quad \rho = 1{\cdot}3862944$,
$f = 0{\cdot}1036802$, $\quad i = 0{\cdot}0551502$, $\quad l = 0{\cdot}3465736$,
$g = 0{\cdot}0687064$, $\quad k = 0{\cdot}0408309$, $\quad m = 0{\cdot}1793226$.

13. The constant numbers which will be wanted, in computing the arithmetical values of A and B, are those denoted by e, h, and ρ, which are given in the preceding Article; and the constant logarithms are the following, which are respectively set down to as many places of figures as are requisite.

$L. 2 = 0{\cdot}3010{,}300,$	$L. \frac{2}{3} = \bar{1}{\cdot}8239{,}087,$	$L. \frac{21}{32} = \bar{1}{\cdot}817,$
$L. \frac{3}{8} = \bar{1}{\cdot}5740{,}3$	$L. \frac{3}{4} = \bar{1}{\cdot}8750{,}6,$	$L. \frac{1}{2} = \bar{1}{\cdot}6989{,}7,$
$L. \frac{5}{8} = \bar{1}{\cdot}796,$	$L. \frac{5}{12} = \bar{1}{\cdot}6197{,}9,$	$L. \frac{9}{16} = \bar{1}{\cdot}750$
$L. f = \bar{1}{\cdot}0157{,}0$	$L. i = \bar{2}{\cdot}7415{,}5$	$L. l = \bar{1}.5398{,}0$
$L. \frac{g}{f} = \bar{1}{\cdot}821$	$L. \frac{k}{i} = \bar{1}{\cdot}869$	$L. \frac{m}{l} = \bar{1}{\cdot}714$
	$L. \pi = 0{\cdot}4971{,}499$*.	

14. An example, to illustrate the method of computing by these theorems, may now be proper.

Let it be required to compute A and B by the 1st and 3d theorems, in Art. 12. when the two planets are Venus and the Earth.

This arithmetical work may stand as follows, in three columns, the logarithms being in the middle, and the numbers corresponding to them on the two sides; where a distinction is made, which is too obvious to need any description. By this arrangement, a frequent repetition of words, number, and logarithm, will be avoided.

* All these constant logarithms are to be written on a slip of paper, for the sake of expedition in the use of them.

First, for the value of A.

	Numbers.		Logarithms.		Numbers.
Here	$a = 1\cdot5236{,}71$	*	$0\cdot1828{,}913$		
and	$b = 1\cdot4451{,}60$	Ar. c	$\bar{1}\cdot8400{,}841$		
	$a - b = 0\cdot0785{,}11$		$\bar{2}\cdot8949{,}305$		
	$a + b = 2.9688{,}31$		$0\cdot4725{,}855$		
	$\frac{a-b}{a+b} = cc$	- -	$\bar{2}\cdot4223{,}450$		
		$\frac{2}{cc}$	$1\cdot8786{,}850$	-	$75\cdot62841 = \frac{2}{cc}$
					$0\cdot19315 = e$
		fcc	$\bar{3}\cdot4380{,}4$	-	$0\cdot00274 = fcc$
		$\frac{f}{g}cc$	$\bar{2}\cdot244$		
		Sum of these two logs.	$\bar{5}\cdot682$	- -	$0\cdot00005 = gc^4$
The sum of these four terms is				- -	$75\cdot82435$

Having now found the value of the four terms $\frac{2}{cc} + e + fcc + gc^4$, we must next find the value of the three logarithmic terms $\alpha + \frac{3}{8}\alpha cc + \frac{3.5}{8.8}\alpha c^4$, which may quickly be done as follows:

Half the logarithm of cc is $\bar{1}\cdot2111{,}725$ = the sine of 9° 21′ 32″·28; and half this angle is 4° 40′ 46″·14, the logarithmic cotangent of which is 1·0869,576; and this common logarithm

* I was favoured with these numbers by Dr. MASKELYNE.

reduced to an hyperbolic logarithm, by Table XXXVII. of DODSON's Calculator, gives - - $2{\cdot}50281 = \alpha$

α -	$0{\cdot}39843$		
$\frac{3}{8}cc$ - -	$\bar{3}{\cdot}99638$		
Sum of these two logs.	$\bar{2}{\cdot}39481$	- -	$0{\cdot}02482 = \frac{3}{8}\alpha cc$
$\frac{5}{8}cc$ - -	$\bar{2}{\cdot}218$		
	$\bar{4}{\cdot}613$	- -	$0{\cdot}00041 = \frac{3.5}{8.8}\alpha c^4$;

The sum of these three terms is - $2{\cdot}52804$; to which, add the sum of the four terms above found, $75{\cdot}82435$, and we have

	$1{\cdot}8940{,}523$	-	$78{\cdot}35239$ = all the terms.
$\pi(a+b)^{\frac{3}{2}}$	$1{\cdot}2060{,}283$		
The difference of these two logarithms is	$0{\cdot}6880{,}240$	-	$4{\cdot}87555 = A$.

The value of A being now found, the computation of the value of B will be very easy, since, of the six terms wanted, two are already computed, and the logarithms of all the rest are at hand. This operation may stand as follows, the logarithms being still in the middle.

		$1\cdot38630 = \varrho$
	$\overline{3}\cdot9621$ -	$0\cdot00916 = lcc$
$\frac{l}{m}cc$	$\overline{2}\cdot136$	
Sum of these two logs.	$\overline{4}\cdot098$ -	$0\cdot00013 = mc^4$
		$5\cdot00562 = 2\alpha$
	$\overline{2}\cdot51975$	$0\cdot03309 = \frac{1}{2}\alpha cc$
$\frac{9}{16}cc$	$\overline{2}\cdot172$	
Sum of these two logs.	$\overline{4}\cdot692$	$0\cdot00049 = \frac{1.9}{2.16}\alpha c^4$
	0·8085,343	$6\cdot43479 =$ { Sum of these six terms.
$\pi(a+b)^{\frac{1}{2}}$	0·7334,427	
Diff. of these two logs.	0·0750,916	$1\cdot18875 = A_{\prime}$
A -	0.6880,240	
a -	0.1828,913	
Sum of these two logs.	0.8709,153 -	$7\cdot42874 = Aa$
	0.7951,840 -	$6\cdot23999 = Aa - A_{\prime}$
$\frac{2}{b}$	0·1411,141	
$\frac{2}{b}(Aa - A_{\prime})$	0·9362,981	$8\cdot63571 = B.$

We have now the values sought, *viz.* A = 4·8756, and B = 8·6357; and from these may the values of C, D, E, &c. be easily found, by the equations given in Art. 20. of the preceding paper. I have set them down here only to five places of figures, it being evident, from the value of $a - b$, that the result cannot be depended on to more places than five, which, however, are very sufficient for the purpose.

I have taken notice above, in the computation of B, that, of six terms wanted, two were ready, one of them being a constant quantity, and the other computed in the preceding operation; and it may be remarked, that, if the two terms in which c^4 enters had been omitted, the difference in the result would not have been so much as 2 in the fourth place of decimals, which is inconsiderable. Therefore, of the six terms in this expression, two are given, and two more only need be computed in this case.

And it may be further remarked, that the term e is always given in the expression of the value of A, and that the two terms in which c^4 enters may be omitted, as that will occasion a difference in the result, of only 3 in the fifth place of decimals, which is quite inconsiderable. Of the seven terms, therefore, in Theorem 1. Art. 12. one is given, and four more only need be computed, when Venus and the Earth are the two planets of which the perturbations are to be computed.

15. My avocations calling me off from these delightful speculations, I must now put an end to this paper, without mentioning some other observations which I have made on this subject.

May 6, 1797.

XXIII. *Account of a Substance found in a Clay-pit; and of the Effect of the Mere of Diss, upon various Substances immersed in it. By Mr.* Benjamin Wiseman, *of Diss, in Norfolk. Communicated by* John Frere, *Esq. F. R. S. With an Analysis of the Water of the said Mere. By* Charles Hatchett, *Esq. F. R. S. In a Letter to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.*

Read April 19, 1798.

THE substance I have inclosed was found near Diss, in a body of clay, from five to eight feet below the surface of the soil. All the pieces I observed laid nearly in a horizontal direction; and varied in size, from two or three ounces, to as many pounds. The colour of the substance, when taken fresh from the clay-pit, was like that of chocolate; it cuts easily, and has the striated appearance of rotten wood. The pieces were of no particular form; in general, they were broad and flat, but I do not recollect to have met with a piece that was more than two inches in thickness: it breaks into laminæ, between which are the remains of various kinds of shells. The specific gravity of this substance, dried in the shade, is 1.588; it burns freely, giving out a great quantity of smoke, with a strong sulphureous smell.

By a chemical analysis, which I cannot consider as very accurate, one hundred grains appear to contain,

	grains.
Of inflammable matter, including the small quantity of water contained in the substance - -	41.3
Of mild calcareous earth - - -	20.0
Of iron - - - - -	2.0
Of earth, that appears to be silex - - -	36.7
	100

On the Effect of the Mere of Diss, upon various Substances.

Observing, several years ago, that flint stones taken out of the Mere of Diss were incrusted with a metallic stain, I was induced to make some experiments, in order to discover the nature or composition of this metallic substance.

Nitrous acid readily removes it, dissolving a part, and leaving a yellowish powder, which, washed and filtered, was found to be sulphur. Vegetable fixed alkali precipitated from the nitrous acid a ferruginous coloured powder, which was iron.

With a view to determine what length of time was necessary for the formation of this metallic stain upon flint stones, or other substances, I inclosed in a brass wire net the following articles: flint stones, calcareous spar, common writing slate, a piece of common white stone ware, and likewise a piece of black Wedgwood-pottery. After remaining in the water from the summer of 1792 to August, 1795, the flints and Wedgwood-ware had acquired the metallic stain in a slight degree, and the slate had assumed a rust colour; the other substances appeared not to be at all altered. I was greatly surprised to find the copper wire that held the net, surrounded with a metallic coating of a considerable thickness; it was of a deep lead colour, and of a granulated texture. When taken from the wire, and ground in a mortar, it had a black appearance, inter-

spersed with very hard shining particles. The wire was evidently eroded, and this substance deposited in the place of the copper that was decomposed, somewhat similar to the decomposition of iron in cupreous waters.

By repeated chemical analysis of this substance, one hundred grains contain, of copper, 70; of sulphur, 16.6; of iron 13.3 grains.

I have never met with an account of the decomposition of copper, in waters impregnated with iron, in any chemical work; and, as iron appears to have a greater affinity to the vitriolic acid than copper has, (as is constantly evinced in the neighbourhood of copper mines,) it appears an anomaly in chemistry, that I am not adept enough in the science to account for.

[The President and Council, thinking the effects of the water of Diss Mere deserving of further inquiry, desired Mr. WISEMAN would send some of the said water, for the purpose of examination. Mr. WISEMAN accordingly sent a quantity of the water, accompanied by the other substances described in the following letter to the President.]

SIR, Diss, May 29, 1798.

As the Society have expressed a wish, through Mr. FRERE, to have some of the water in which the copper wire was deposited, which Mr. FRERE, at my request, laid before the Society, I have sent two gallons of the water of Diss Mere, (No. 1.) with a small quantity of copper cuttings, (No. 2.) which laid in the same water, a few feet from the side, and six feet in depth, from the 7th of February, 1797, to the 20th of the present month, May, 1798. The pieces of copper, when laid in, weighed 3051 grains; when they were taken out, and washed from the mud that lightly adhered to them, preserving and weighing the scaly matter that came off, they weighed 2944 grains, indicating a loss of 107 grains. Examining the pieces of copper, the same evening they were taken out of the water, I observed a number of small crystals formed upon some of them, in the form of pyramids joined at their bases; these crystals lost their shining appearance, by the evaporation of the water of crystallization, in the warmth of the succeeding day. Whether they will be preserved in a journey of nearly 100 miles, is perhaps doubtful. No. 3. contains two pieces of copper, on which the crystals were most abundant. No. 4. contains a small quantity of the substance formed upon the copper, that came off in washing and in weighing it.

The town of Diss is principally situated on the NNE and E sides of this piece of water. The land runs pretty steep on the W and N of it, to the height of 40 or 50 feet: on the SE, the ground comes within a few feet of the level of it. The soil of the upper part of the town is a stiff blue clay; that of the lower part, to the SE, a black sand, beneath which it is a moor.

The water in the higher parts of the town is good; in the lower parts, it is a chalybeate, of which a specimen is sent, (No. 5.)

No. 6. contains a quantity of flint stones, taken from the SE side of the Mere, where the water is shallow; many of which are strongly marked with the metallic stain, which they acquire by lying in this water a few years.

The Mere contains about eight acres, and is of various depths, to twenty-four feet: from its situation with respect to the town, it may naturally be supposed to contain a vast quantity of mud, as it has received the silt of the streets for ages. In summer, the water turns green; and the vegetable matter that swims on its surface, when exposed to the rays of the sun, affords vast quantities of oxygen gas. I cannot help considering this process as having a considerable agency in the corrosion, and in the formation of the metallic crust upon the copper deposited in this water. Some of this vegetable matter will be found in the water sent to the Society.

I intend to make some further experiments with different metallic substances, at different parts, and at various depths; but, as the process is slow, if in the mean time you, Sir, or any of the members of the Society, will have the goodness to point out any experiment you or they may wish to have made, I shall be very glad to contribute all in my power towards the illustration of the subject.

I have the honour to be, &c.

BENJ. WISEMAN.

The Right Hon. Sir JOSEPH BANKS, Bart.
K. B. P. R. S.

[The water, and other substances described in the foregoing letter, were delivered to Mr. HATCHETT, who had been previously requested, by the President and Council, to examine them. The result of his examination is related in the following letter to the President.]

Analysis of the Water of the Mere of Diss. By Charles Hatchett, *Esq.*

DEAR SIR, Hammersmith, Sept. 14th, 1798.

In consequence of the request which you and the Council of the Royal Society have done me the honour to make, that I would examine the water of Diss Mere, and the other substances sent by Mr. WISEMAN, I now hasten to acquaint you with the result of my experiments.

The substances sent by Mr. WISEMAN are as follows:

Some copper wire, with a blackish grey incrustation.

Water from Diss Mere, (marked No. 1.)

Copper cuttings, covered with a blackish crust, similar to that on the copper wire, (marked No. 2.)

Some cuttings similar to those abovementioned, (marked No. 3.)

The paper, No. 4. contained some of the black crust, detached from the cuttings.

No. 5. A quart bottle, containing some water from the lower part of the town of Diss, and called, by Mr. WISEMAN, a chalybeate water.

No. 6. Some flints, taken from the SE side of the Mere, where the water is shallow, and having (as Mr. WISEMAN terms it) a metallic stain.

My first experiments were made on the incrustation of the copper wire, mentioned in Mr. WISEMAN's first letter.

This incrustation was easily detached from the wire, and, being reduced to powder, was digested with nitro-muriatic acid, in a gentle heat: a green solution was formed, and there remained a residuum, of a pale yellow, which proved to be sulphur.

The solution being diluted with two parts of distilled water, was supersaturated with pure ammoniac, by which, a few brown flocculi of iron were precipitated. The supernatant liquor was blue; and, being evaporated, and redissolved by sulphuric acid, the whole was precipitated by a plate of polished iron, in the state of metallic copper. The component parts of this coating were therefore copper, and a very small portion of iron combined with sulphur.

I could not extend these experiments, as the whole quantity of the coating that I was able to collect, amounted only to three grains and an half.*

The next experiments were made on the black crust of No. 2, 3, and 4.

This I found to be exactly the same as that formed on the copper wire; *viz.* it consisted of copper combined with sulphur, and a very small portion of iron.

* The copper wire, when the coating was removed, was perfectly flexible, and the surface did not appear unequal or corroded: this is commonly the case under such circumstances; for, when sulphur has combined superficially with a metal, the compound is observed to separate easily, so as to leave the metal underneath not injured in quality, and very little, if at all, affected in appearance. Those who diminish silver coin, make use of the following method.

They expose the coin to the fumes of burning sulphur, by which a black crust of sulphurated silver is soon formed, which, by a slight but quick blow, comes off like a scale, leaving the coin so little affected, that the operation may sometimes be repeated twice or thrice, without much hazard of detection, if the coin has a bold impression.

I next examined the water of Diss Mere, (No. 1.) and I was at length led on, step by step, to make a regular analysis of the fixed ingredients.

Before I made the analysis, I examined this water with certain re-agents, and remarked the following properties.

1. The water of Diss Mere has a yellowish tinge, and the flavour is rather saline; but it has not any perceptible odour.

2. Prussiate of potash did not produce any effect.

3. Acetite of lead produced a slight white precipitate.

4. Nitrate of silver formed one, very copious.

5. Tincture of galls had not any effect.

6. Muriate of barytes caused a slight precipitate.

7. Ammoniac, potash, and oxalic acid, severally produced precipitates, when added to different portions of this water.

ANALYSIS.

A. Three hundred cubic inches of the water, by a gentle evaporation, left a pale brown scaly substance, which weighed 58 grains.

B. These 58 grains were digested in alcohol, without heat, during 24 hours, and afforded a solution, which, by evaporation, yielded muriate of lime, slightly tinged by marshy extract, 18 grains.

C. Six ounces of distilled water were then poured on the residuum, and, with repeated stirring, remained during 24 hours. By evaporation, this afforded muriate of soda, with a very small portion of sulphate of soda; in all, 10 grains.

D. What remained was boiled in 800 parts of distilled water, and the solution, being evaporated, left of selenite 1.70 gr.

E. The undissolved portion now weighed 25 grains, and was digested with diluted muriatic acid: a great part was dissolved, with much effervescence, and, being filtrated, afforded, by ammoniac, of alumine 1.50 gr. From this, I afterwards separated a very minute quantity of iron, by means of prussiate of potash.

F. Carbonate of soda was then added to the liquor, and precipitated carbonate of lime 21 grains.

G. The insoluble residuum weighed 3.50 gr.; and proved to be principally carbon, (produced by decomposed vegetable matter,) with a very small quantity of siliceous earth.

The result of this analysis was, therefore,

	grains.	
B. Muriate of lime - - -	18	
C. Muriate of soda, with a very small portion of sulphate of soda - - -	10	
D. Selenite - - - -	1	70
E. Alumine, with a portion of iron too small to be estimated - - - -	1	50
F. Carbonate of lime - - -	21	
G. Carbon, with a little siliceous earth -	3	50
	55	70
Loss	2	30
	58	0

It is worthy of notice, that the iron present was in so very small a quantity as not to be detected by any test, till it had been separated in conjunction with the alumine.

The water No. 5, from Mr. WISEMAN's account, does not appear to have been concerned in producing the effects which he has observed, and the quantity was too small to be subjected to a regular analysis, I noted, however, what follows.

1. It has a very strong hepatic flavour and smell.

2. A plate of polished silver, put into it, became black in a few hours.

3. It became faintly bluish with prussiate of potash, after standing five or six hours.

4. Tincture of galls produced a faint purple cloud.

5. Solution of acetite of lead afforded a brown precipitate.

6. Nitrate of silver produced the same.

7. Potash, and ammoniac, caused a precipitate; but that of the former was the most copious.

8. Oxalic acid produced a precipitate.

9. Muriate of barytes had also a slight effect.

The water No. 5. cannot, therefore, be considered as a chalybeate, (the quantity of iron contained in it being scarcely perceptible;) but it appears to be a water containing some hepatic gas, together with substances similar to those contained in No. 1.

From the above experiments it is evident, that the water No. 1. does not contain any of the component parts of the crust formed on the copper wire and cuttings, although it is certain that the incrustation took place during the immersion of those bodies; but, before I mention my ideas on this subject, I shall give an account of some experiments made on the flints, No. 6. These were coated with a yellowish shining substance, which appeared to me to be pyrites; and, as the flints could not have contributed any metallic substance to form this coating, I was enabled by their means to ascertain, whether the copper of the crust, formed on the wire and cuttings, had been furnished by the pieces of copper, or by any thing in the vicinity of the water.

1. I poured nitro-muriatic acid on some of the flints, in a matrass, so as completely to cover them.

The coating was rapidly dissolved, with much effervescence; and, when the flints appeared perfectly uncoated, and in their usual state, I decanted the liquor.

2. A yellow matter subsided, which proved to be sulphur.

3. Prussiate of potash produced Prussian blue; and the remaining part of the solution, being supersaturated with ammoniac, afforded an ochraceous precipitate of iron.

The supernatant liquor did not become blue, as when copper is present, nor was the smallest trace of it afforded by evaporation.

Martial pyrites is, therefore, the only substance deposited on bodies immersed in the water of Diss Mere; and the copper of the crust, formed on the wire and cuttings, was furnished by those bodies.

It is proved by the analysis, that the water of Diss Mere does not hold in solution any sulphur, and scarcely any iron; it has not, therefore, been concerned in forming the pyrites; but it appears to me, that the pyritical matter is formed in the mud and filth of the Mere; for Mr. WISEMAN says in his letter, that "the Mere has received the silt of the streets for ages." Now it is a well known fact, that sulphur is continually formed, or rather liberated, from putrefying animal and vegetable matter, in common sewers, public ditches, houses of office, *&c. &c.*; and this most probably has been the case at Diss. Moreover, if sulphur, thus formed, should meet with silver, copper, or iron, it will combine with them, unless the latter should be previously oxidated.

The sulphur has therefore, in the present case, met with iron, in, or approaching, the metallic state, and has formed pyrites; which (whilst in a minutely divided state, or progres-

sively during formation,) has been deposited on bodies, such as the flints, when in contact with the mud.

But an excess of sulphur appears to be present; for, when copper is put into the Mere, the sulphur readily combines with it; and, at the same time, a small portion of iron appears to unite with the compound of copper and sulphur, possibly by the mere mechanical act of precipitation.

The incrustation on the copper wire and cuttings is, in every property, similar to that rare species of copper ore, called by the Germans *Kupfer schwärtze, (Cuprum ochraceum nigrum;)* and I consider it as absolutely the same. In respect to the martial pyrites on the flints, there can be no hesitation; and, as in these two instances, there were evident proofs of the recent formation of ores in the humid way, I was desirous to ascertain the effect on silver. I therefore wrote to Mr. WISEMAN, to request that he would take the trouble to make the experiment; and received from him the following answer, accompanied by the specimens.

"SIR, Diss, 8th September, 1798.

"Immediately upon the receipt of your letter, (27th July,) I laid some silver plate, and silver wire, into the Mere; the whole weighed 235.6 gr. I took it out on Thursday last, (Sept. 6th) and, after cleaning it carefully from mud and weeds, I find it weighs 242.8 gr.; an increase of 7.2 gr.

"The silver plate you will find much tarnished, in some parts almost black; the wire is in many places fairly incrusted, which crust, upon the pressure of the fingers, comes off in thin scales. The whole appearance of the silver strongly indicates

the presence of sulphur, which I have no doubt abounds in every part of the Mere.

"The peculiar smell of the mud gives me reason to suppose, that a great deal of hepatic air is produced; which, probably, uniting with the iron held in solution in the water of the Mere, may account for the martial pyrites found on the flints.

"By what affinity the copper wire, laid in this water, is attacked, I am not chemist enough to determine.

"I have begun a set of experiments, with the view of producing the same effects upon copper wire by artificial means; but whether I shall succeed, I am not able at present to say.

"I am, *&c.*

"BENJ. WISEMAN."

P. S. By experiments I have lately made, I find hepatic gas precipitates carbonate of iron in the form of a black flocculent matter; 71 parts of which are iron, and 29 sulphur.

The silver plate I found (as Mr. WISEMAN has mentioned) much tarnished, and in many places almost black, but I could not detach any part of it. I succeeded better with the wire, and collected a small portion of a black scaly substance, which, as far as the smallness of the quantity would allow it to be ascertained, was sulphuret of silver; and was similar, in every respect, to the sulphurated or vitreous ore of silver, called by the Germans *Glasertz.*

This effect on the silver was to be expected; and I recollect to have read, not many months ago, in one of the foreign journals, that Mr. PROUST had examined an incrustation, of a dark grey colour, formed in the course of a very long time, on some silver images, in a church at (I believe) Seville. This incrustation he found to be a compound of silver with sulphur, or, in other words, vitreous silver ore.

The same principle is the cause of the tarnish which silver plate contracts with so much ease, particularly in great cities; for this tarnish is principally a commencement of mineralization on the surface, produced by the sulphureous and hepatic vapours dispersed throughout the atmosphere, in such places.

To Mr. WISEMAN's observations we are much indebted, as they make known the recent and daily formation of martial pyrites, and other ores, under certain circumstances. It is not to be supposed that such effects are local, or peculiar to Diss Mere; on the contrary, there is reason to believe that similar effects, on a larger scale, have been, and are now, daily produced in many places.

The pyrites in coal mines have, probably, in great measure thus originated.

The pyritical wood also may thus have been produced; and, by the subsequent loss of sulphur, and oxidation of the iron, this pyritical wood appears to have formed the wood-like iron ore which is found in many parts, and particularly in the mines on the river Jenisei, in Siberia.

In short, when the extensive influence of pyrites in the mineral kingdom, caused by the numerous modifications of it, in

the way of composition and decomposition, is considered, every thing which reflects light on its formation becomes interesting; and I cannot but regard as such, the effects which Mr. WISEMAN has observed in the Mere of Diss.

With great respect, I remain, &c.

CHARLES HATCHETT.

The Right Hon. Sir JOSEPH BANKS, Bart.
K.B. P.R.S. &c.

XXIV. *A Catalogue of Sanscrita Manuscripts presented to the Royal Society by Sir* William *and Lady* Jones. *By* Charles Wilkins, *Esq. F. R. S.*

Read June 28, 1798.

1. *a.* MAHA'-BHA'RATA.*

A poem in eighteen books, exclusive of the part called *Raghu-vansa;* the whole attributed to *Crishna Dwaipáyana Vyása;* with copious notes by *Nila-canta.* This stupendous work, when perfect, contains upwards of one hundred thousand metrical verses. The main subject is the history of the race of *Bhárata,* one of the ancient kings of India, from whom that country is said to have derived the name of *Bhárata-varsha;* and more particularly that of two of its collateral branches, distinguished by the patronymics, the *Cauravas* and the *Pauravas,* (so denominated from two of their ancestors, *Curu* and *Puru,*) and of their bloody contentions for the sovereignty of *Bhárata-varsha,* the only general name by which the aborigines know the country we call *India,* and the Arabs and Persians *Hind* and *Hindostan.* But, besides the main story, a great variety of other subjects is treated of, by way of introduction and episode. The part entitled *Raghu-vansa,* contains a distinct history of the race of *Crishna.* The *Mahá-bhárata* is so very popular throughout the East, that it has been translated into most of its numerous dialects; and there is an abridgment of it in the Per-

* The Sanscrita words are spelt according to the method practised by Sir WILLIAM JONES, in his works.

sian language, several copies of which are to be found in our public libraries. The *Gitá*, which has appeared in an English dress, forms part of this work; but, as it contains doctrines thought too sublime for the vulgar, it is often left out of the text, as happens to be the case in this copy. Its place is in the 6th book, called *Bhishma-parva*. This copy is written in the character which, by way of pre-eminence, is called *Déva-nágari*. Ly J.

1. *b*. *Ditto*.

Another copy, without notes, written in the character peculiar to the province of *Bengal*, in which the *Brahmans* of that country are wont to transcribe all their *Sanscrita* books. Most of the alphabets of India, though they differ very much in the shape of their letters, agree in their number and powers, and are capable of expressing the *Sanscrita*, as well as their own particular language. This copy contains the *Gitá*, in its proper place. Ly J.

2. *a*. *Rámáyana*.

The adventures of *Ráma*, a poem in seven books, with notes, in the *Dévanágari* character. There are several works with the same title, but this, written by *Válmici*, is the most esteemed. The subject of all the *Rámáyanas* is the same: the popular story of *Ráma*, surnamed *Dásarathi*, supposed to be an incarnation of the god *Vishnu*, and his wonderful exploits to recover his beloved *Sitá* out of the hands of *Rávana*, the gigantic tyrant of *Lancá*. Ly J.

2. *b*. *Ditto*.

Another copy, in the *Bengal* character, without notes, by *Válmici*. Ly J.

2. *c*. *Ditto.*

A very fine copy, in the *Dévanágari* character, without notes; but unfortunately not finished, the writer having been reduced to a state of insanity, by habitual intoxication. S. W. J.

3. *a*. *Sri Bhágavata.*

A poem in twelve books, attributed to *Crishna Dwaipáyana Vyása*, the reputed author of the *Mahá-bhárata*, and many other works; with notes by *Sridhara Swámi*. *Dévanágari* character. It is to be found in most of the vulgar dialects of India, and in the Persian language. It has also appeared, in a very imperfect and abridged form, in French, under the title of *Bagavadam*, translated from the *Támul* version. The chief subject of the *Bhágavata* is the life of *Crishna*; but, being one of that species of composition which is called *Purána*, it necessarily comprises five subjects, including that which may be considered the chief. The Bráhmans, in their books, define a *Purána* to be "*a poem treating of five subjects: primary creation, or creation* "*of matter in the abstract; secondary creation, or the produc-* "*tion of the subordinate beings, both spiritual and material;* "*chronological account of their grand periods of time, called* "*Manwantaras; genealogical rise of families, particularly of* "*those who have reigned in India*; *and, lastly, a history of the* "*lives of particular families.*" There are many copies of this work in England. Ly J.

3. *b*. *Ditto.*

Another copy, in the *Bengal* character, without notes.

Ly J.

3. *c*. *Ditto.*

Another copy, on palm leaves, in the *Bengal* character.

S. W. J.

4. *Agni Purána.*

This work, feigned to have been delivered by *Agni*, the god of fire, contains a variety of subjects, and seems to have been intended as an epitome of Hindu learning. The poem opens with a short account of the several incarnations of *Vishnu;* particularly in the persons of *Ráma*, whose exploits are the theme of the *Rámáyana*, and of *Crishna*, the material offspring of *Vasudéva.* Then follows a history of the creation; a tedious dissertation on the worship of the gods, with a description of their images, and directions for constructing and setting them up; a concise description of the earth, and of those places which are esteemed holy, with the forms of worship to be observed at them; a treatise on astronomy, or rather astrology; a variety of incantations, charms, and spells, for every occasion; computation of the periods called *Manwantaras;* a description of the several religious modes of life, called *A'srama*, and the duties to be performed in each of them respectively; rules for doing penance; feasts and fasts to be observed throughout the year; rules for bestowing charity; a dissertation on the great advantages to be derived from the mystic character OM! with a hymn to *Vasishta.* The next subject relates to the office and duties of princes; under which head are given rules for knowing the qualities of men and women; for choosing arms and ensigns of royalty; for the choice of precious stones; which are followed by a treatise on the art of war, the greatest part of which is wanting in this copy. The next head treats of worldly transactions between man and man, in buying and selling, borrowing and lending, giving and receiving, *&c. &c.* and the laws respecting them. Then

follow certain ordinances, according to the *Véda*, respecting means of security from misfortunes, *&c.* and for the worship of the gods. Lists of the two races of kings, called the *Suryavansa*, and the *Chandravansa;* of the family of *Yadu*, and of *Crishna;* with a short history of the twelve years war, described in the *Mahá-bhárata*. A treatise on the art of healing, as applicable to man and beast, with rules for the management of elephants, horses, and cows; charms and spells for curing various disorders; and the mode of worshipping certain divinities. On the letters of the *Sanscrita* alphabet; on the ornaments of speech, as applicable to prose, verse, and the drama; on the mystic signification of the single letters of the *Sanscrita* alphabet; a grammar of the *Sanscrita* language, and a short vocabulary. The work is divided into 353 short chapters, and is written in the *Bengal* character. L^y. J.

5. *Cálica Purána.*

A mythological history of the goddess *Cálí*, in verse, and her adventures under various names and characters; a very curious and entertaining work, including, by way of episode, several beautiful allegories, particularly one founded upon the motions of the moon. There seems to be something wanting at the end. *Bengal* character, without notes. L^y. J.

6. *a. Váyu Purána.*

This work, attributed to *Váyu* the god of wind, contains, among a variety of other curious subjects, a very circumstantial detail of the creation of all things celestial and terrestrial, with the genealogy of the first inhabitants; a chronological account of the grand periods called *Manwantaras, Calpas*, &c.; a description of the earth, as divided into *Dwipas, Varshas*, &c.,

with its dimensions in *Yojanas;* and also of the other planets, and fixed stars, and their relative distances, circumferences of orbits, *&c. &c.* Written in the *Dévanágari* character. Ly. J.

6. *b. Ditto.*

A duplicate in the *Dévanágari* character. Ly. J.

7. *Vriban Náradiya Purána.*

This poem, feigned to have been delivered to *Sanatcumára*, by the inspired *Nárada*, like others of the *Puránas*, opens with chaos and creation; but it treats principally of the unity of God, under the title of *Mahá Vishnu;* arguing, that all other gods are but emblems of his works, and the goddesses, of his powers; and that the worshipping of either of the triad, creator, preserver, or destroyer, is, in effect, the worshipping of him. The book concludes with rules for the several tribes, in their spiritual and temporal conduct through life. It is a new copy, in the *Bengal* character, and, for a new copy, remarkably correct. Ly. J.

8. *Náradiya Purána.*

This poem treats principally on the worship of *Vishnu*, as practised by *Rukmángada*, one of their ancient kings. *Dévanágari* character. S. W. J.

9. *a. Bhavishyóttara Purána.*

The second and only remaining part. The subject is confined to religious ceremonies. *Dévanágari* character. S.W.J.

9. *b. Ditto.*

With an Index. *Dévanágari* character. Ly. J.

10. *Gita-góvinda.*

A beautiful and very popular poem, by *Jayadéva*, upon *Crishna*, and his youthful adventures. *Bengal* character. Ly. J.

11. *a. Cumara Sambhava.*

An epic poem on the birth of *Cártica,* with notes, by *Calidása. Dévanágari* character. The notes are separate. Ly. J.

11. *b. Ditto.*

A duplicate of the text only, in the *Bengal* character. Ly. J.

12. *Naishadha.*

The adventures of *Nala;* a poem, with notes. *Bengal* character. Ly. J.

13. *Bhatti.*

A popular heroic poem, in the *Bengal* character. Ly. J.

14. *Raghu-vansa.*

The race of *Crishna,* a poem by *Calidas,* with notes. *Dévanágari* character. Ly. J.

15. *Vrihatcathá.*

Tales in verse, by *Somadéva. Dévanágari* character. Ly. J.

16. *Singhásána.*

The throne of *Rájá Vicramáditya;* a series of instructive tales, supposed to have been related by thirty-two images which ornamented it. *Dévanágari* character. It has been translated into Persian. Ly. J.

17. *Cathá Saritságara.*

A collection of tales by *Somadéva. Dévanágari* character. Ly. J.

18. *Suca Saptati.*

The seventy tales of a parrot. *Dévanágari* character. S.W.J. The Persians seem to have borrowed their *Tuti-náma* from this work.

19. *Rasamanjari.*

The analysis of love, a poem, by *Bhánudatta Misra. Dévanágari* character. Ly. J.

20. *Sántisataca.*

A poem, in the *Bengal* character. Ly. J.

21. *Arjuna Gitá.*

A dialogue, something in the manner of the *Bhagavat Gitá. Dévanágari* character. Ly. J.

22. *Hitópadésa.*

Part of the fables translated by C. W. Written in the *Bengal* character. Ly. J.

23. *Brahmá Nirupana.*

On the nature of *Brahmá. Dévanágari* character. Imperfect. Ly. J.

24. *Mégbaduta.*

A poem. *Bengal* character. Ly. J.

25. *Tantra Sára.*

On religious ceremonies, by *Crishnánanda Battáchárya. Bengal* character. S. W. J.

26. *Sahasra Námá.*

The thousand names of *Vishnu. Dévanágari* character. S. W. J.

27. *Cirátárjuniya.*

A poem, in the *Bengal* character. Ly. J.

28. *Siddhánta Siromani.*

A treatise on geography and astronomy, by *Bháscaráchárya. Dévanágari* character. S. W. J.

29. *Sangita Náráyana.*

A treatise on music and dancing. *Dévanágari* character. S. W. J.

30. *Vrihadáranyaca.*

Part of the *Yajur Véda*, with a gloss, by *Sancara. Dévanágari* character. Ly. J.

31. *Nirucli*, or *Nairucta*.

A gloss on the *Véda*. *Dévanágari* character. L^{y}. J.

32. *Aitaréya*.

A discourse on part of the *Véda*. *Dévanágari* character. L^{y}. J.

33. *Chandasi*.

From the *Sáma Véda*. *Dévanágari* character. L^{y}. J.

34. *Mágha Ticá*.

A comment on some other work. *Dévanágari* character. L^{y}. J.

35. *Rájaballabha*.

De materia Indorum medicâ; by *Náráyanadasá*. *Bengal* character. L^{y}. J.

36. *Hatha Pradípaca*.

Instructions for the performance of the religious discipline called *Yóga;* by *Swátmáráma*. *Bengal* character. L^{y}. J.

37. *a*. *Mánava Dharma Sástra*.

The institutes of *Mănŭ*, translated into English by S. W. J. under the title of "*Institutes of Hindu Law, or the Ordinances* "*of Menu.*" *Dévanágari* character. Incorrect. L^{y}. J.

37. *b*. *Ditto*.

Duplicate in the *Dévanágari* character. Very incorrect. L^{y}. J.

38. *Mugdha-bódha-ticá*.

A commentary on the *Mugdha-bódha*, which is a *Sanscrita* grammar, peculiar to the province of Bengal, by *Durgá Dása*. *Bengal* character. Four vols. L^{y}. J.

39. *Sáraswati Vyácarana*.

The *Sanscrita* grammar called *Sáraswati*. (That part only which treats of the verb.) *Dévanágari* character. L^{y}. J.

40. *Sárávali.*

A grammar of the *Sanscrita* language. Incomplete. *Bengal* character. S. W. J.

41. *Siddhánta Caumudi.*

A grammar of the *Sanscrita* language, by *Panini*, *Cátyáyana*, and *Patanjali*; with a duplicate of the first part, as far as compounds. *Dévanágari* character. L^y. J.

42. *a. Amara Cósa.*

A vocabulary of the *Sanscrita* language, with a grammatical comment. Not perfect. *Dévanágari* character. L^y. J.

42. *b. Ditto.*

The botanical chapter only, with a comment. *Dévanágari* character. L^y. J.

42. *c. Ditto.*

The whole complete. *Bengal* character. S. W. J.

43. *Médini Cósa.*

A dictionary of the *Sanscrita* language. *Dévanágari* character. L^y. J.

44. *Viswapracása Cósa.*

A dictionary of the *Sanscrita* language; by *Mahéswara*. *Dévanágari* character. L^y. J.

45. *Sabda Sandarbha Sindu.*

A dictionary of the *Sanscrita* language; by *Cásináthá Sarman*. It appears from the introduction, that it was compiled expressly for the use of S. W. J. The learned author is, at present, head professor in the newly-established college at *Varanási*. *Dévanágari* character. Two vols. folio. L^y. J.

46. *Vénisanhára.*

A drama, *Sanscrita* and *Prácrita*, in the *Bengal* character. L^y. J.

47. *Mahá Nátaca.*

A drama, *Sanscrita* and *Prácrita,* in the *Bengal* character.

Ly. J.

48. *Sacuntalá.*

A drama, *Sanscrita* and *Prácrita,* in the *Bengal* character. This is the beautiful play which was translated into English by S. W. J. but not the copy he used for that purpose.

Ly. J.

49. *Málati and Mádhava.*

A drama, *Sanscrita* and *Prácrita,* in the *Bengal* character.

Ly. J.

50. *Hásyárnava.*

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51. *Cautuca Sarvaswam.*

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55. *Manavicágnimitra.*

A drama, *Sanscrita* and *Prácrita.* *Bengal* character.

Ly. J.

56. A catalogue of *Sanscrita* books, on various subjects. *Dévanágari* character. L^{y}. J.

N. B. Those articles in the above catalogue, marked S.W.J. were presented by Sir William Jones; and those marked L^{y}. J. by Lady Jones. A catalogue of the Persian and Arabic MSS. presented by them, will be given in a future volume.

PRESENTS

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